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EDITED BY

J. MARK BALDWIN
PRINCETON UNIVERSITY

AND

J. McKEEN CATTELL
COLUMBIA COLLEGE

WITH THE COÖPERATION OF

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THE REVIEW, having entered upon its second volume, ventures to call attention to the following partial list of American University Professors and Instructors who have written for it. Their articles comprise all departments of psychological work:—

Harvard:
Wm. James.
Hugo Münsterberg.
Josiah Royce.
Geo Santayana.
C. W. Pierce.
Herbert Nichols.
Josiah Pierce.

Yale:
Geo. T. Ladd.
G. S. Fullerton.
D. G. Brinton.
G. S. Dodge.
Livingston Wheeler.
[illegible] Seashore.

Chicago:
John Dewey.
H. H. Donaldson.
C. A. Strong.
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Columbia:
N. M. Butler.
J. McK. Cattell.
M. A. Starr.
H. F. Osborn.
J. H. Hyslop.
Livingston Farrand.

Princeton:
A. T. Ormond.
J. Mark Baldwin.
J. G. Hibben.
H. C. Warren.

Other University teachers have contributed from Michigan, Wisconsin, Clark, Cornell, Wellesley, Brown, Wesleyan, Indiana, Iowa, Lake Forest, Stanford, Bryn Mawr, Smith, Illinois, Miami, Bellevue Medical, and the Catholic University of Washington. Foreign writers for the Review have included:

Carl Stumpf, Berlin; James Sully, London; Shadworth Hodgson, London; [illegible], Paris; J. Philippe, Paris; A. Kirschmann, Toronto; F. Krüger, Leipsic.

The first issue of an annual Bibliography of Psychological Literature called *The Psychological Index* is now ready. It comprises titles of the literature of Psychology and cognate subjects in all languages for 1894. It may be ordered from Macmillan & Co., price 75 cents, or, when ordered by a subscriber to the Psychological Review, 50 cents. The *Index* is prepared by Dr. Livingston Farrand of Columbia College, and Mr. H. C. Warren of Princeton, in coöperation with the editors of the Review.

The Review also announces the founding of a series of *Monograph Supplements*, consisting of longer dissertations, researches, &c., to be issued at intervals as they may be accepted by the editors. They will be numbered consecutively and 5–6 numbers will constitute a volume. The first of these *Monograph Supplements* is now ready. It is a research on *Sensations from Pressure and Impact* by Dr. Harold Griffing. (pp. 86, price 75 cts.)

These *Monographs* may be ordered from the same publishers at the rate (by subscription) of $5 a volume of about 600 pages; or by single numbers, at from 50 cents to $1.00, according to the number of pages. Papers for publication in this series during 1895 may be sent to Prof. J. Mark Baldwin, Princeton, N. J.

VOL. II. No. 4. JULY, 1895.

THE PSYCHOLOGICAL REVIEW.

THE PSYCHOLOGY OF PAIN.

BY PROFESSOR C. A. STRONG,

University of Chicago.

The theory of pleasure and pain, so ably advocated by Mr. Marshall,[1] certainly represents a widespread opinion among the competent in our department. Among its upholders are Wundt and Höffding, Külpe and Lehmann, Sully and Bradley, besides prominent members of this association.[2] According to this theory, pleasure and pain are not independent mental contents, capable of existing in consciousness alone, but a side or aspect of other contents—a sort of modification or coloring of sensations and ideas. In Prof. James's happy phrase, they are 'mere manners of experiencing' these other contents. Wherever we feel a pain, there we have a sensation or idea, distinct from the pain, with reference to which the pain is felt. Furthermore, we never have a sensation or idea which is not felt with some degree of pleasure or pain. So that in every actual state of mind we are able to distinguish these two sides, the cognitive and the affective. The affective side of a sensation is called its feeling-tone, and feeling-tone is conceived as a necessary attribute, to be classed with quality and intensity.

The following quotations will serve to make the doctrine clearer. Sully remarks that "pleasure and pain do not occur as isolated experiences, but in close connection with presen-

[1] *Pain, Pleasure, and Æsthetics*, London, 1894.

[2] Read before the American Psychological Association at its Princeton meeting.

tative elements."[1] Bradley says: "Without sensation we never have pleasure or pain. Not a pleasure, but something pleasant is what we experience. . . . If we like to apply the term aspect, or side, or moment, these are all open to objection, as metaphors must be. But what they try to say is that . . . . pain and pleasure do not exist apart from sensation, any more than duration or intensity are ever discovered by themselves."[2] And Mr. Marshall holds that pleasure and pain are "differential qualities of all mental states, of such nature that one of them must and either of them may, under their proper conditions, belong to any element of consciousness."[3]

Mr. Marshall calls this the quale theory of pleasure and pain, but it seems to me that a more readily intelligible name would be the aspect theory, and I shall so designate it in this paper. What I propose to do is to inquire whether this theory gives a correct account of a single concrete experience, that of physical pain. And the kind of physical pain I shall especially consider is pain felt through the skin. But the whole question suffers from ambiguity. The word pain signifies not only (1) the feeling we have when the skin is cut or burnt, that which the Germans call *Schmerz;* but also (2) the feeling of displeasure excited in us by this and other experiences, that which the Germans call *Unlust.* Now I am aware that the aspect theory is primarily a theory of displeasure. So far as this is so, I am not concerned with it in this paper. I agree that displeasure is always felt with reference to a content distinct from itself. But the aspect theory regards pain as the highest degree of displeasure, and holds that the pain of a cut or a burn can always be analysed into a tactile or temperature sensation on the one hand and a feeling of displeasure on the other. These are the doctrines I wish especially to examine. That pain usually calls forth displeasure I do not question; but that pain *is* displeasure, and is always felt with reference to a tactile or temperature sensation, is what I am inclined to doubt. I

[1] *The Human Mind*, Vol. II, p. 7.
[2] *Mind*, No. XLIX, p. 2. *Cf.* Wundt, *Phys. Psych.*, 3d ed., Vol. I, p. 509 infra.
[3] *Pain, Pleasure, and Æsthetics*, p. 3.

propose, therefore, to take up the experience of cutaneous pain, and to look at it first from the neurological and afterwards from the introspective point of view, with the aim of ascertaining whether the account of it given by the aspect theory is correct.

I.

The question of special pain terminations I shall not discuss; for, now that Goldscheider has withdrawn his former claims on their behalf,[1] there remains little competent testimony to their existence. I pass at once to the important question of the psychological bearing of the condition known as analgesia. It has been maintained that this condition demonstrates the existence of special paths for pain, and so disposes of the aspect theory: let us inquire how far this is true.

1. Analgesia, as every one knows, signifies loss of sensibility to pain without necessary loss of tactile or temperature sensibility. Schiff produced this condition in animals by sectioning the gray matter of the spinal cord and leaving the posterior columns of the white matter intact; though I believe that physiologists question the rigor of his experiments. Analgesia occurs at a stage in the action of cocaine on the skin, and in that of ether or chloroform on the nerve-centres —the result being that the patient feels the touch of the surgeon's knife without feeling the pain of the cut. It also occurs in certain spinal diseases. In syringo-myelia, for example, pain and temperature sensations are lost, while tactile sensations remain normal. In locomotor ataxia, a frequent symptom is analgesia of the legs; and such patients often get burns or sores without noticing, or even allow a fractured bone to go untreated, because they feel no pain.

But the most striking cases occur in hysteria. "Some women," says Dr. Weir Mitchell,[2] "remain for years without the pain sense. In one case of mine a hysterical paralytic recovered really useful health, and except herself and

[1] *Ueber den Schmerz*, Berlin, 1894.
[2] *Medical Record*, Dec. 24, 1892.

myself no one knows that she cannot be hurt by knife or fire or a blow. The interior organs still feel pain as usual. All other forms of skin sense are as keen as ever. This woman used to hurt or burn herself from want of care; now she has learned to take heed. I have seen many such, but only one other where the general health was as competent. She doubts now whether she would accept anew the natural condition of the pain sense."

In the same address Dr. Mitchell mentions a remarkable case of natural analgesia which deserves to be brought to the attention of psychologists. It is reported by a Georgia physician, Dr. Paul Eve, and is that of a man said never to have known pain. Dr. Eve says he knew the patient personally, and was for years intimate with his family physicians, who often spoke of Mr. A.'s peculiar incapacity to feel pain. Mr. A. was about 56 years old at the time of his death. He was a corpulent man, weighing about 250 pounds, and had been a free liver. He was a lawyer by profession, of good intellect, a man of strong mind and body, and had acquired a considerable reputation as an advocate and politician. During a political campaign, not liking the appearance of a finger which had been injured in an affray, he bit it off himself and spat it upon the ground. He had at one time an ulcer on a toe which resisted treatment for nearly three years; he told his physician at the time, and repeated the statement later, that from first to last it never gave him any pain. He also had at one time an abscess in his hand, involving the whole forearm and arm, which became enormously swollen and threatened his life; the lancet had to be freely used; yet during the whole treatment he said he experienced no pain. He said he felt no pain when his eyes were operated on for cataract; and Dr. Eve says he can vouch for his statue-like immobility during the second operation. Only during his last illness did he complain of pain for a time, but passed into his usual insensible condition before he died. "It is proper to say," observes Dr. Eve, "that Mr. A. was a man of great probity, and never boasted of being insensible to pain."

2. Analgesia, however, is only one among a number of

similar conditions, the so-called partial anæsthesias. Thus we may have the converse condition of tactile anæsthesia, or temperature anæsthesia, or both, without analgesia. Schiff produced this condition in animals by sectioning the posterior columns of the cord and leaving the gray matter intact. It is said to be producible by anointing the skin with acetic or carbolic acid.[1] Finally, it occurs in nervous disease. Mosler[2] has recorded the case of a woman with brain disease whose right side was insensitive to touch, though pain and temperature sensations remained normal. The prick of a pin caused distinct pain, yet she did not feel the laying-on of the entire hand. When a fold of skin was lifted between the fingers and severely pinched, she was aware of what had happened through the pain alone. In a case of locomotor ataxia, also recorded by Mosler, the prick of a pin caused pain everywhere, yet on the left leg the pressure sense was so dulled that the patient could not tell the difference between 100 and 500 grams, nor even feel their weight on the skin.

3. The facts thus far given seem to justify the conclusion that the senses of touch and of pain are independent of each other. But they likewise indicate that the sense of temperature is independent of both. For where pain is lost and touch retained, temperature is sometimes retained and sometimes lost; and, conversely, where touch is lost and pain retained, temperature is sometimes retained and sometimes lost. Indeed, it is probable that the temperature sense must be divided into a sense of heat and a sense of cold; for cases are on record in which cold sensations were lost and heat sensations retained, and other cases in which heat sensations were lost and cold sensations retained.[3]

To sum up: The conclusion generally drawn by pathologists is that the skin possesses four distinct forms of sensibility, namely, touch, cold, heat and pain, and, that any one or any combination of these may be lost or impaired without

[1] Beaunis, *Les Sensations Internes*, p. 214.
[2] *Berliner Klinische Wochenschrift*, 1868.
[3] See Foster, *Physiology*, 5th ed., Part III, p. 1101.

detriment to the rest. The nerve-impulses which awaken these different kinds of sensation are assumed to pass upward by distinct paths in the spinal cord, and the partial anæsthesias are explained in an anatomical way by the blocking of these paths. Whether this explanation is the true one we shall see later on.

Let us at present consider how the aspect theorists meet the facts of partial anæsthesia. They do so in a very simple manner, namely, by holding that the fourth sense is not properly a pain sense, but a sense whose normal product is what Mr. Marshall calls the cutting-pricking sensation, and that it is the exaggeration of this sensation which gives rise to pain. What is lost, according to Mr. Marshall, in analgesia is the cutting-pricking sense with its pain-giving capacity, while the tactile and temperature senses with their pain-giving capacities remain unimpaired.[1]

This seems a not unnatural interpretation of the facts. But when we go on to ask what *are* the pain-giving capacities of the tactile and temperature senses alone, the answer is one of some importance to the aspect theory. For the pain-giving capacities of these senses alone appear to be practically zero. The analgesic will endure a violent blow on the skin, or the contact of ice or of a red-hot iron, without the slightest trace of discomfort. So that, if the aspect theorist still holds that tactile and temperature sensations in themhave an affective coloring, he must at least admit that this selves coloring never amounts to positive pain.

Observe the consequences of this admission. One of them is that, when we get a painful burn, and suppose the pain to be a modification or aspect of the heat, we are under an illusion. For the pain and the heat are called forth by separate nerve-fibres. Or when we feel a painful pressure, and suppose the pain to be an attribute of the pressure, we are under an illusion. The pain can at most be an attribute of the cutting-pricking sensation, if we admit its existence. But what is this cutting-pricking sensation, if we mean by it the feeling of being cut or pricked? Is it not a tactile sensa-

[1] *Pain, Pleasure, and Æsthetics*, pp. 19, 20.

tion, due to the inevitable stimulation of tactile terminations which just precedes the prick or the cut? If Mr. Marshall refers to anything else, I confess he seems to me to be wilfully assuming a new kind of sensation, without sufficient introspective evidence, for the sake of a theory.

It seems to me (speaking from the point of view of the aspect theory) that Mr. Marshall would have done well in this matter had he followed the example of the judicious Foster, whose doctrine his own so closely parallels. Foster[1] agrees with Goldscheider[2] that "we have no evidence that simple stimulation of the retina, however excessive, will give rise to pain—meaning by pain the kind of sensation we feel when the skin is cut or burnt. . . . We have no evidence that an auditory or an olfactory or a gustatory sensation can, through mere intensity, become converted into a sensation of pain. . . . We may assume that the pain which we feel when the finger is cut is a wholly different thing from the pain which is given to the most delicately musical ear by even the most horrible discord." And these considerations suggest to Foster that cutaneous pain is not simply an exaggeration of tactile or temperature sensations, but a separate sensation developed in a different way. But even on the assumption that this difference of pain-giving capacity is not one of kind, but only one of degree, it is surely important to emphasize the distinction between the senses which are normally analgesic and those which are not. These remarks I make hypothetically, from the point of view of the aspect theory, which for reasons soon to be explained I am unable to accept.

Goldscheider,[3] while agreeing with Foster that sight, hearing, taste, and smell are analgesic, thinks it unnecessary to assume a fourth set of cutaneous fibres, and refers cutaneous pains to the sense of touch. He believes that the temperature sense is analgesic, while the sense of touch is not; and bases this opinion on the fact that pains can be pro-

[1] *Physiology*, 5th ed., Part IV, pp. 281, 282.
[2] *Ueber den Schmerz*, p. 8.
[3] *Ibid*, p. 13.

duced by stimulating the pressure spots but not the heat and cold spots. How far this view is a necessary inference from his observations I cannot judge, but there are further facts of nervous disease which seem to contradict both it and the view of Foster, and, indeed, to be quite irreconcilable with the interpretation of the partial anæsthesias thus far given. I refer, first, to a symptom of locomotor ataxia recently put forward by one of Prof. Starr's assistants, Dr. Skinner, and consisting in a hyperalgesia of temperature without accompanying hyperalgesia of touch.[1]

The tests were made by heating or cooling water in a test-tube and holding the tube against the skin. For warmth the water was heated to 120° F. (50° C.), for cold it was cooled to 50° F. (10° C.)—temperatures quite supportable to a normal nervous system, but productive of pain to these tabetics. Out of 24 cases examined, the tests showed the presence of hyperalgesia to both heat and cold in three, and of hyperalgesia to heat alone in one. In one of the three cases the symptoms were very marked, the patient starting and uttering an exclamation of pain when the test-tube was applied to the skin. In this case there was analgesia of both legs to pricking and pinching, and *the hyperalgesia to temperature was present over the analgesic areas, and over these areas only*. The heat was always recognized as heat and the cold as cold. In another case there was no disturbance of cutaneous sensibility except the hyperalgesia to temperature, and this was greater to cold than to heat—so great, in fact, that the patient begged to have the test-tube removed at once, although the temperature of the water was only 58° F. (15° C.).

I may remark in passing that this interesting symptom is not so new as Dr. Skinner supposes. Dr. Bolko Stern mentions it distinctly in an article in the *Archiv für Psychiatrie* for 1886. He noticed in certain cases a manifest hyperalgesia to contact with cold objects, in persons who bore painful stimuli on normal skin-areas with patience. One woman started and cried out on being touched with cold vessels

[1] Starr, *Familiar Forms of Nervous Disease*, pp. 173–175.

which caused no unpleasant sensation to normal persons. Dr. Stern was, however, unable to discover a clear case of hyperalgesia to warmth.

Both Dr. Stern and Dr. Skinner mention that ataxic patients are often very sensitive to cold. Thus Dr. Skinner mentions a patient who could not dress warmly enough for comfort, and who kept his room at a temperature of 90° F. (32° C.), yet still felt chilly all the time; and another who had felt continual chilliness for five years. These cases seem to differ only in degree from those of marked hyperalgesia. They are interesting as bridging the gulf between pathological and normal sensitiveness to cold.

I am indebted to Dr. Charles W. Burr of Philadelphia for directing my attention to a second symptom of nervous disease which, while the converse of the former, seems like it to show that tactile pains are distinct from temperature pains. In certain cases of syringo-myelia, the patient's foot is normally sensitive to pricking and pinching, but totally analgesic to temperature stimuli such as ice or a hot iron.

Here, then, we have facts which seem to show that the paths of temperature pains are distinct from those of pains due to pricking and cutting; and even that the paths of heat pains are distinct from those of cold pains. They likewise seem to show that temperature pains are more closely bound up with normal sensations of temperature than would be the case if pain impulses were conducted by a special set of fibres. But how can this be reconciled with the apparent teaching of the partial anæsthesias?

Possibly it might be by the assumption that these ataxic temperature pains are a pathological exaggeration of the mild and harmless affective coloring which the aspect theorist supposes to attend the functioning of the temperature sense alone. But when we consider that this would oblige us to explain the pains of the ataxic by one sense of fibres, and those of the normal person when the test-tube is heated a few degrees hotter by another set, the explanation appears somewhat forced and unnatural. We are moved to return to the partial anæsthesias and attempt a physiological explana-

tion of them, by assuming that tactile and temperature fibres are each capable of carrying two kinds of impulses, ordinary impulses and pain impulses, and that either of these capacities may be lost as a result of disease. But this explanation is promptly negatived by the experiments (if we may trust them) of Schiff, which appear to prove that moderate impulses pass upward through the white matter of the cord and excessive impulses through the gray matter.

An hypothesis in regard to analgesia, which promises to help us out of our difficulties, was published by Wundt in the first edition of his *Physiologische Psychologie*, and has been repeated in subsequent editions.[1] Wundt assumes that in the peripheral nerves the paths of pain impulses are the same as those of touch, heat and cold impulses. He sees no reason for assuming a special pain sense, or a fourth sense of any kind. But he thinks that, when tactile or temperature impulses reach the cord, they find two paths open—a primary path, probably leading through the white matter, and a secondary path or paths, leading through the gray matter. Impulses of moderate intensity take the primary path, and this path can accommodate only moderate impulses. When excessive impulses come, they overflow into the secondary paths and pass upward through the gray matter.

This hypothesis, which has the countenance of Funke and of Goldscheider,[2] will be found to explain most of the above-mentioned facts. Hypnotic analgesia, for instance, may be due to a temporary, hysterical analgesia to a more permanent, lowering of the excitability of the gray matter, the effect being to block the paths. Observe, first, that this hypothesis provides more or less distinct paths in the gray matter for tactile pains and for temperature pains. That these paths should be entirely distinct is not required by the facts; it would suffice if the two kinds of impulse entered the gray matter at different points or passed to different centres, which could undergo lesion separately. Observe, secondly, that the hypothesis does not exclude the

[1] 3rd ed., Vol. I, pp. 115, 116; 4th ed., Vol. I, pp. 111, 112.
[2] *Ueber den Schmerz*, p. 19.

existence of a fourth cutaneous sense: although it invalidates the arguments for it drawn from the condition of analgesia. And, of course, if such a sense be assumed, it is no longer necessary to distinguish between its pain-giving capacity and that of the other cutaneous senses; since analgesia is now explained by the blocking of the excessive impulses which enter the gray matter, not by the abolition of this fourth sense.

Finally, it must be admitted that this hypothesis is not free from difficulties. It requires us to explain the action of cocaine and that of acetic and carbolic acid on the skin by a differential paralysis, now of the pain-carrying capacity of the nerve-fibre, and now of its touch-carrying capacity. The hypothesis also seems inconsistent with the fact that pain cannot be produced by stimulating the heat and cold spots; though, perhaps, it could be produced if enough of them were stimulated simultaneously. Finally, it ignores the consideration by which Foster seeks to establish an analogy between touch and the higher senses, though it is not inconsistent with the analgesic character of these senses themselves.

But the question which chiefly concerns us is as to the bearing of all these neurological facts and theories on the psychology of pain. This may be summed up as follows:

(1) The evidence seems on the whole to indicate that pain impulses are exaggerations of tactile, heat, and cold impulses, and are conducted inward by the same nerve-fibres; and this seems favorable to the aspect theory.

(2) The analgesic condition seems to be one of indifference, so far as the remaining cutaneous sensations are concerned, and this seems hostile to the aspect theory. The affective cream seems to be taken cleanly off, leaving a mere skim-milk of cognition. But this, again, may be disputed, and it may be claimed that the currents which pass upward through the gray matter awaken a sensation with a strong feeling-tone, and that this is now lost; while those which pass upward through the white matter awaken a sensation with a weak feeling-tone, and that this still remains.

It seems to me that with this claim we have reached the

limit of what can be learned from neurology. It is evident that the aspect theory does not stand or fall with the existence or non-existence of special pain nerves or of special paths for pain in the spinal cord.

II.

Let us now turn to the introspective analysis of pain, and inquire whether the aspect theory gives a correct account of the subjective facts. This theory regards feeling-tone as an attribute of every sensation, and compares it to intensity. Is the comparison a just one? And, in the first place, is feeling-tone a *necessary* attribute of every sensation? A sensation is unthinkable without both quality and intensity—it must be a definite kind of sensation, and there must be a definite amount of that kind. Is a sensation unthinkable without feeling-tone? So far is this from being true, that a large proportion of our ordinary sensations are practically without it. No doubt most of these are attended with some minimal degree of feeling, but this is so slight as to be unappreciable.

This state of things has been interpreted by the aspect theorists in two ways:

(1) According to Wundt, these apparently feelingless sensations do not prove that feeling-tone is not a necessary property; they have a feeling-tone, but their feeling-tone is zero; it is one of indifference.[1] This is as if one should say that every tactile sensation has some temperature quality, but that when the contact is neither hot nor cold the temperature quality is one of indifference. But temperatures neither hot nor cold are pure contacts; and in the same way sensations neither pleasant nor unpleasant are not sensations with a feeling-tone of zero, but sensations without any feeling-tone at all.

(2) Other aspect theorists admit this, but hold that there are no such feelingless sensations, that all sensations have at least a minimal feeling-tone. To this view I have nothing whatever to say; nor do I see how it can be either substan-

[1] *Physiologische Psychologie*, 3rd ed., Vol. I, pp. 290, 508–9.

tiated or overthrown by introspection. I will merely add two remarks: First, that the demand that all sensations should be accompanied by at least a minimum of pleasure or pain is a theoretical demand, based on the prior assumption that feeling-tone is a necessary attribute, analogous to quality and intensity; secondly, that the recognition of a zero-point is equivalent to the admission that theoretically this attribute is not a necessary one.

Let us now pass to the opposite end of the scale, and consider the case of sensations (to use the Wundtian terminology) with a maximal feeling-tone. The aspect theory holds that in physical pain we always have tactile, temperature or other sensations present 'in painful phase,' 'with a coloring of pain.' Thus Lehmann says that "a feeling, whether of pleasure or of pain, never occurs apart from a sensation however weak, and in every case where such an isolated feeling is supposed to have been observed, the sensational element has merely been overlooked."[1] And Külpe, in his recent book, says: "The characteristic feature of pain is not the sensational quality, which is never absent, whether this be great heat, or strong pressure, or a screaming noise, or a blinding light, but the feeling of the disagreeable, of which pain is the highest degree."[2] Here we have the orthodox Wundtian doctrine. But elsewhere, after observing that pain occurs almost solely in connection with common sensations, Külpe goes on to remark that the analysis of these "is rendered difficult by the fact that strong feelings cover up (*verdecken*) sensational qualities they accompany";[3] and in another passage he refers to "the well-known fact that pain usually overpowers (*übertäubt*) the accompanying sensational qualities," and remarks that "if pressure, heat, or cold is intensified until it becomes painful, the impression is in all three cases of essentially the same kind."[4] With this we may compare the statement of E. H.

[1] *Hauptgesetze des menschlichen Gefühlslebens*, p. 33.
[2] *Grundriss der Psychologie*, p. 93.
[3] *Ibid.*, p. 152.
[4] *Ibid.*, p. 327.

Weber that "the pain produced by heat or cold is very different from the sensation of heat or cold. If the pain is not extreme, we feel at the same time the heat or cold which causes it, and can then distinguish pain due to heat from pain due to cold. But if it is extreme, the sensation is the same, whether caused by heat or cold."[1] Or, as Prof. James tersely expresses it, "heat, cold, and pressure are indistinguishable when extreme—we only feel the pain."[2]

If we attempt to render these familiar facts in Wundtian phraseology, we can do so only by saying that in extreme physical pain the feeling-tone has become so excessive as to overpower and cover up the quality and intensity of the sensation. We appear to have left a sensation with an intense feeling-tone, but without any quality or intensity. And yet these three are supposed to be necessary attributes of every sensation. I know Külpe insists that the sensational quality is never absent, but this directly contradicts his statement that it is overpowered by the pain, and that pain caused by pressure, heat, and cold have the same quality. Indeed, he is himself more or less conscious of the contradiction, for he follows up the statement last referred to with the words: "Here we are of course conceiving pain not as feeling-tone, but as a peculiar quality of sensation."[3] And elsewhere he refers to the possibility that "we have to recognize in pain, apart from the displeasure (*Unlust*) therein contained, a special class of sensation, called forth by the intense stimulation of every sensory nerve."[4]

Now this, it seems to me, is the only doctrine which accurately reflects the facts, and it is incompatible with the aspect theory, which classes feeling-tone with intensity. This classification is illegitimate, because at the bottom of the scale we have quality and intensity without any feeling-tone, or with next to no feeling-tone, while at the top of the scale we have feeling-tone in great intensity, but no quality.

[1] *Tastsinn und Gemeingefühl*, p. 118.
[2] *Briefer Course*, p. 68.
[3] *Grundriss der Psychologie*, p. 327.
[4] *Ibid.*, p. 248.

In other words, what the aspect theorist calls feeling-tone is in reality a kind of quality. *As the quality of warmth becomes intense it becomes mixed with the quality of pain, and when the pain is extreme there is no warmth left.* Only in the intermediate sensations is it possible to draw a distinction between a cognitive and an affective side of the experience ; at one extreme all is cognition, at the other all is feeling. And throughout the scale there is an opposition between the two sides, such that one can predominate only at the expense of the other. Thus Sully tells us that "the affective and the cognitive element do not appear with equal prominence in our sensational and ideational experience" ; that in organic sensation and emotion the affective preponderates over the presentative, while in visual perception and ordinary thought the presentative preponderates over the affective.[1] Is such an opposition conceivable between the quality and the intensity of a sensation? Can the intensity of a sensation cover up and conceal the quality? No more than the breadth of a smile can conceal the face. The quality and the intensity are given with equal prominence, because they are only two different ways of classifying a single unique content. Indeed, so pregnant a fact is this opposition between cognition and feeling, that, if the extremes of indifference and pure pain were not to be met with in experience, this opposition would alone suffice to prove the incorrectness of the theory which classes feeling-tone with intensity.

The aspect theorist may be said to make the same mistake as a person who, viewing the series of colors between red and yellow, should describe the yellow apparently visible in orange as a sort of tone, and the red visible in it as the sensational basis to which this tone was attached. The phrase hedonic coloring proves nothing, but it shows how well the facts would lend themselves to a sensational interpretation.

To make my doctrine perfectly unambiguous, I must distinguish between cases where the mingled qualities are due to different nerve-fibres, and cases where they are due to

[1] *The Human Mind*, Vol. II, pp. 10, 11.

the same nerve-fibre. It is quite probable, even on our theory of the partial anæsthesias, that in the experience of a painful burn the pain is due to different nerve-fibres from the heat. For it is reasonable to suppose that the area of skin actually burnt is surrounded by a margin in which only heat impulses are produced. At the same time, it can hardly be doubted that in cases of moderate pain both the pain and the heat may be due to the same nerve-fibre. Let us suppose a fibre or a set of fibres to be stimulated with such intensity as to cause slight pain, yet not so intensely as to obscure the normal quality of heat. Now, to determine the relation between the heat and the pain in cases like this is, of course, the strict object of this paper. And I submit that this sensation of painful heat is to be conceived as a mixture of two co-ordinate qualities, heat and pain. When I say a mixture, I do not mean to imply a composite character in the sensation itself, any more than the taste of lemonade is composite. But, just as the taste of lemonade resembles that of lemon-juice on the one hand and that of sugar on the other, so the sensation of painful heat resembles the pure quality of heat on the one hand and the pure quality of pain on the other.

My conclusion is, then, that pain is as distinctly the content of certain cutaneous sensations as blue of certain visual ones. It seems to me that Bain is right in classing hunger, thirst, nausea, fatigue, and pain with sensations;[1] that Delbœuf is right when he says that "fatigue and sensation are phenomena of the same nature and comparable to each other"[2]; that Münsterberg is right when he says that "sensible pleasure and pain are not extreme degrees of the agreeable and the disagreeable, but sensations, which regularly call forth strong feelings of the agreeable and the disagreeable"[3]; and that Dr. Nichols is right in so far as he places physical pleasure and pain "on a common footing with our other senses as fundamental elements of mind."[4]

[1] *The Senses and the Intellect*, pp. 102, 103.
[2] *Éléments de Psychophysique*, p. 46.
[3] *Beiträge*, Heft IV, pp. 216, 217.
[4] *Philosophical Review*, Vol. I, p. 404.

Or my conclusion may be briefly expressed in the following form. If a new-born babe were subjected through all its senses to stimulations of excessive intensity for the space of an hour, the sole content of its consciousness during that time would be the feeling of pain, nor would this feeling have any connection with or reference to cognitive states whatever.

III.

In conclusion I will reply to an objection and indicate two consequences.

I am sure some of my hearers will be inclined to admit that pain is an independent mental content, not necessarily attached to any other element of consciousness, but yet will deny that it is on that account to be classified with sensations. They will say that in this classification an important character of pain has been overlooked, one which differentiates it totally from the states we call sensations. Sensations, that is to say, are projected outside the body and elaborated into perceptions of objects, of which they then appear as the qualities; whereas pain is not thus projected or elaborated, but remains throughout a purely subjective state.

It is perfectly true that pain is a subjective state in this sense. But be it observed that such elaboration into objects is a subsequent operation, performed upon the sensation as it were from without, and not necessarily implying an essential difference between it and the feeling of pain simply as mental elements, apart from their differing fate in this respect. The considerations already brought forward suffice to prove that originally and in themselves feelings of pain and ordinary sensations are of the same nature. For both are called forth by nerve-currents from without; and both are substantive mental contents, capable of existing in consciousness alone, though commonly present together. The only difference between them is that sensations are found in experience to vary with their stimuli, and therefore to furnish information about the stimuli, whereas feelings of pain do not so vary, and therefore tell us only of ourselves. Bradley puts the matter clearly and forcibly when he says:

"Like sensations they [pleasure and pain], are at first neither objective nor subjective. . . . That pleasure or pain, as they come first, have, in any sense whatever, a reference to the Ego is a fundamental error. It takes the products of development and places them at the starting-point, where no Ego . . . exists except in false theory."[1] And I could match this passage with another from Prof. Wundt.[2]

The same point may also be stated in the following way. Feelings of temperature are sometimes due, not to external objects, nor even to the contact of the surrounding air, but to the state of the circulation, to the increased or diminished development of animal heat. If they were always due to this cause, if they varied as little with the stimulus and as much with the organ as do feelings of pain, we should account heat and cold no less subjective than we now account pain.

I pass to the two consequences.

(1) It is often said that pleasure and pain are not localized, but that what is localized is the sensations they accompany.[3] But we have seen that often there are no such accompanying sensations, and yet we know that the pain is localized. When I burn my finger, it is not merely the heat sensation that is localized there, it is the pain as well. A person whose hand is stimulated behind a screen may not be able to distinguish the pain of a burn from that of a cut, but he feels the pain in his hand.

(2) It is often said that we cannot have images of pleasure or pain, or that, if we do imagine them, we actually begin to feel these states anew.[4] It seems to me that, so far as this statement is true, it applies to other images as much as to images of pain. The theory that images have a permanent identity and recur, instead of being re-created, may be said to have received its deathblow. If images of pain are faint and unreal, the same is the case with images of taste and smell. I will not assert with Hume that images

[1] *Mind*, No. XLIX, p. 2.
[2] *Vorlesungen über Menschen und Thierseele*, 2nd ed., p. 226 infra.
[3] Sully, *The Human Mind*, Vol. II, p. 7 infra.
[4] Marshall, *Pain, Pleasure and Æsthetics*, pp. 29, 30.

are merely weakened sensations; but I do assert that whatever is true of images generally is true of images of pain.

Finally, let me repeat that my criticisms are directed, not against the aspect theory as a theory of displeasure, but against the aspect theorists' analysis of physical pain. I hold that physical pain is not a compound of an indifferent sensation with a feeling of displeasure, but itself a sensation which calls forth displeasure.

EXPERIMENTAL INDUCTION OF AUTOMATIC PROCESSES.[1]

BY PROFESSOR W. R. NEWBOLD,
University of Pennsylvania.

That processes analogous to those that normally accompany consciousness do at least sometimes take place in connection with a human brain, without being represented in the consciousness properly belonging to that brain, is now generally admitted; but the full import of the admission to psychology is not clearly understood. In the first place, it is not certain whether these processes are accompanied by consciousness or not. If not, we must suppose that the cortical process, as such, is not alone sufficient to produce a mental state, but needs the coöperation of some other factor. This hypothesis bears too much resemblance to the old soul theory to meet with favor in contemporary psychology, and we find that most writers claim that these dissociated processes are accompanied by true conscious states, which are related to the complex of similar states that constitutes the personal consciousness of the individual much as the consciousness of some other person is related to it. Furthermore, it is supposed that these 'parasitic states' are subject to precisely the same laws as those that govern the ordinary 'upper consciousness' of the individual in question. They may develop, take to themselves associative helpmeets, and finally form a subconscious dream, which may persist for a considerable time and produce sundry disagreeable phenomena in the consciousness of its involuntary host. In extreme cases the mental parasite may become so complex and highly organized as to constitute a true 'secondary personality' in all respects analogous to the original or upper personality; and it may at times displace the upper person-

[1] Read at the Princeton meeting of the American Psychological Association.

ality and assume temporary control of the motor system, thus becoming manifest to other persons. Pierre Janet, who has developed this doctrine more consistently than any one else with whom I am acquainted, regards such parasitic idea systems as essentially pathological phenomena. But a somewhat analogous conception is being developed among those interested in 'psychical research,' according to which all thoughts, memories, hopes and fears which have been dismissed from the upper consciousness still exist in conscious form, and even organized into a self-conscious being in what is termed 'the subliminal self,'—and some think that the 'subliminal self' is sensitive to influences which are lost upon the normal consciousness.

Among the phenomena frequently ascribed to such subconscious automatism are the 'phantasms of the glass' and automatic or 'spirit' writing-phenomena of which the 'professional psychologist' usually knows less than he should. Indeed, so far as I know, no professional psychologist has made any study of the phantasm of the glass from the purely psychological point of view, and but little has been done with automatic writing. I have been conducting some rather desultory experiments in these lines for the past two years, and, although the results are not sufficiently exact for purposes of computation, they have raised in my mind no little doubt as to their supposedly subconscious origin.

Let us first examine the phantasm of the glass. It has been known since the dawn of history—and no one can say how long before—that certain individuals, while looking into a transparent or reflecting medium, see therein hallucinatory scenes and figures which were supposed to emanate from the unseen world of spirit. From the Urim and Thummim of the Jewish highpriest to the 'Crystal' of Dr. Dee, we find this belief among all ages and peoples. Within the last few years the Society for Psychical Research has undertaken to look into this ancient superstition and see whether it be based upon any residuum of fact.

Those who believe in the existence of conscious states dissociated from the normal upper consciousness of the individual, are inclined to regard the phantasm of the glass as

the product of subconscious automatism, brought by the transparent medium within the ken of the upper consciousness; and it cannot be denied that many cases are on record which would seem to point to some such origin. But in my own series of experiments I found no reason to make this assumption. I commonly used in my experiments a glass ball made for the purpose, but I found that a glass of water or a small mirror reflecting a white surface answered the purpose quite as well. Such a medium gives the patient a vision of unfilled space, and its function appears to be simply that of an irritant to the highly organized visual mechanism. This seems borne out by the fact that the phantasm is more likely to appear when the medium is well illuminated. Usually some interval elapses before any effect is produced. In a few cases the phantasm was seen upon the first glance into the medium; more commonly one must wait from five seconds to five minutes. In one case the image appeared after the lapse of twenty minutes. The first symptoms of a response on the part of the central visual mechanism to the exciting stimulus are frequently found in the appearance of visual sensations of a rather indeterminate character. The medium becomes opaque, being apparently filled with smoky or milky masses; sometimes small masses of white, like minute clouds, drift rapidly through it. At other times these prodromal phenomena take the form of flushes of color —red, blue or yellow. More seldom yet, the medium seems to become more brilliantly illuminated just before the phantasm emerges. The phantasm sometimes appears suddenly, but more often is slowly developed out of some of the sense material already present. The cloud-masses take definite shape and then become colored, or the vague blur or spot becomes a nucleus upon which the image develops. One of my subjects, a young girl who visualized well, described it in *naïve* fashion: "You see," said she, "the gray spot seems to sink down to the bottom of the glass and turns and whirls about slowly; then, of course, it has to become something." "But," said I, who do not visualize at all, "why *must* it become something?" "Well," she said, "of course it could not possibly stay a spot; it has to

become something clear or else go out." When there are no indeterminate prodromal phenomena the phantasm sometimes develops out of the reflections on the surface of the glass, but more commonly the reflections interfere with its development, as do all other sensory distractions.

In the case of good 'seers,' the prodromal phenomena appear within five or ten seconds, and the image is fully developed within a minute or two. It is usually brilliantly colored and resembles a minute picture. Sometimes it is indistinct, sometimes the outline is distinct but lacks coloring. Occasionally the picture is perfectly clear and brilliantly colored, but is imperfect. One of my subjects, for example, saw a portion of the full-page cartoon in a recent number of *Puck*, representing the American school system in the clutches of the Popish tiger. The head, forequarters and forepaws of the tiger and a part of his victim were perfectly clear, but the hindquarters were altogether lacking. This patient was a good visualizer, and I asked him to externalize the remainder of the picture and unite it to the image in the glass, but he could not. He said the phantasm had a vividness and externality which images voluntarily externalized never attained. The duration of the image varies, but seems to bear some relation to the length of time it took to produce it. Generally it lasts but a few seconds, but I have seen cases in which it lasted several minutes.

The images are often drawn from the subject's recent visual experience but are often unrecognized. Many of the latter are purely imaginary, but some are doubtless forgotten memories. In one case I found that I could revive memory by hypnotizing the patient and asking him to remember. Once he saw in the glass the face of a young girl which he described in detail. He had never seen her before, he said. Next came a little dog, which he remembered having seen that day in a country postoffice; it came in with its mistress. He could not describe the mistress—he had not seen her face clearly—but was quite sure she was not the woman he had seen in the glass; she was much older. When hypnotized and told to describe the dog's mistress, he described the face he had just seen, and remembered that he

had caught a glimpse of her face as she passed him. At another time he saw a bust of some white material, but it had no pedestal or other surroundings, and he could not remember where he had seen it. When hypnotized he described the pedestal, the stuccoed wall behind it, the wooden floor upon which the pedestal stood, and himself standing before it; but further than this the picture would not develop.

Often the successive images seem to have no relation to one another. When they are related, however, it is nearly always by the law of similarity, seldom, as in the case above quoted, by contiguity. Sometimes it seems as if certain elements in an image persisted and formed the nucleus upon which the new image took shape, thus making it seem as if the images melted into one another. This bears heavily against those who claim that similarity is an ultimate law, and is analogous to the case reported by Mr. Galton in his *Inquiries into Human Faculty*.

The relation of the phantasm to simultaneous sensory states also goes to show that it is a temporary creation and is not the product of independent subconscious processes. Usually it is most intimately related to the sensations which collectively constitute the medium. The least movement of the medium tends to destroy it. The introduction of any visual sensations other than those proceeding from the medium usually destroys it. Magnifying the medium usually destroys it. Upon closing the eyes it is usually not seen; when the eyes are again opened it either has disappeared or is more or less faded. Such phenomena go to show that the phantasm is a mere illusion, constructed upon the sensory basis furnished by the medium. Sometimes, however, it seems to attain a degree of independence which would lend color to the notion that it has been subconsciously originated. I have sometimes found that, when once fully developed, it persisted when the eyes were closed, and even after the removal of the medium. With one subject it proved so durable that he was able to project it upon a sheet of white paper and trace its outlines. But voluntarily externalized images and after-images sometimes prove as permanent, and no one will claim for them a subconscious origin.

Like most hallucinations, the phantasm of the glass is quite independent of the idea-trains of the upper consciousness. Suggestion in the waking state seldom affects it, although it is readily amenable to hypnotic suggestion. In one case only have I found it capable of being affected by voluntary effort. One of my patients told me that while experimenting at home alone he heard the whistle of a locomotive. He forthwith fell to wishing that the locomotive would appear in the glass, and in a few moments it did. The smokestack, upper part of the boiler and a part of the tender appeared, but the picture remained incomplete, and by no amount of wishing could he force it to develop further.

Mr. F. W. H. Myers has called attention to the very interesting fact that if a story be related to a hypnotized patient, and it be suggested that he will see it in the glass, it will appear and be acted out in dramatic form. Many of my readers will remember that Mr. Myers gave illustrations of this before the International Congress in London three years ago. It occurred to me that it would be interesting to show that the glass would bring such a tale to light *without* any direct suggestion, and I tried to do it with a patient named Tom. He is a very ignorant man, but a good hypnotic subject. The experiment gave only negative results. He looked steadily for some time and saw nothing. I then asked, 'Don't you see so and so?' mentioning the first item of the scene he was to see. This slight suggestion proved sufficient and he saw the whole story acted out. I then hypnotized him again and said, "Tom, do you remember the old Greek story—how the giants piled one mountain on another and climbed up to heaven to pull the old god down from his throne?" Tom did not remember. I then bade him look in the glass, and he began to describe the event as he saw it, while I took his words down. He spoke slowly, with frequent pauses, and often moved his head or the medium about as if to get a better view. "It seems like there were some men in there—big giants or something—not very plain what they are—they've got little crowns on, olden style—got on long night-shirts, not very long, sort of cut short, you know, sort of lightish color—there are four

of them—they're moving—piling stuff up—can't tell what it is; guess it's rocks or stuff—whole lot of rocks; they're piling it up in the air—keep building and building up higher like—guess they're trying to reach as high as they can get. Pretty near as high as they can go now—they've got weapons with them, spears and such. Guess they're building a mountain on a mountain—away up now in the clouds. There's somebody else up there—some other man, I guess—looks as if they'd got hold of him and were pulling him off a throne or something. That's all I can see now." (I ask what the old man looks like,) "Dressed like wearing a pair of short pants with tights around his loins—breastplates on him, looks if he were a king or something. Don't see the fellow they pulled off—they are still there and have spears and weapons. Now they're gone too—pile of rocks is still there—now it's going too—it's all gone. Glass is clear." While telling this story the patient seemed to be in a hypnotic condition, as he commonly is while performing a posthypnotic suggestion, but was not suggestible after finishing, nor did he fall asleep as usual. If we could show that the glass tends to externalize suggestions given in hypnosis, it would go to show that there is some organic relation between the realms of consciousness laid bare in hypnosis and those reached by the glass. But at present I see no reason for assuming that there are any such realms existing permanently and subconsciously in all individuals. It is more congruent with the facts to suppose that we are dealing with more or less dissociated mental elements, between which there may be relation whatever, unless we 'set the switches' by suggestion and thus create one.

One would suppose, from *a priori* considerations, that the good visualizer would be more likely to see these images than others, and, so far as my facts go, this would seem to be the case. I tried 86 persons and got phantasms in 22 cases. Twenty of the 22 were young girls. and all were good visualizers. It would then appear that young girls who visualize well are the best 'seers.' But, on the other hand, 46 of my 86 patients were young girls under 22 years of age, pupils in a school where I was lecturing; and of the

remaining 40 not a half-dozen were above 30. Moreover, having early found an apparent relation between visualizing and 'crystal-vision,' I took pains to experiment with all the good visualizers I could find. Upon such selected material one can base no generalizations.

Before turning to other types of automatism, I will quote from my note-book two typical series of phantasms.

Miss E., age 20, well educated girl of a quiet and retiring disposition. Good visualizer. Has had visual hallucinations while in apparently good health. Begins looking in the glass at 9.13 A. M. [Figures indicate interval between entries.] In two minutes 50″, glass grows smoky; 2′ 40″, the smoke is forming into something; cannot make out what it is; 30″, the thing is moving around but does not settle into anything; 1′ 30″, it has become a little brownie, peaked cap and usual costume; is running; the legs move but the figure does not move from its place in the glass; 45″, it is gone; glass is still cloudy; 15″, looks very like the Coliseum; 20″, it is the Coliseum, but there is a little door below which she never noticed before; 10″, something else is coming; "What can it be?" she says; 'it looks like an animal and also like a human being;' 30″, it is a little old man with a great red nose. The Coliseum is still partly visible; the old man has long nose, sharp eyes, thin neck; 40″, the old man has disappeared, the glass is still smoky, the Coliseum still dimly visible in outline. What now, an alligator? No, not that; 10″, the old man back again; he looks younger now, but it is the same man; 30″, seems to be winking, his brows seem to go up and down; 15″, he seems to be turning around and looking the other way; 10″, he is gone; 5″, there he is again; 15″, he is still looking the other way. She has never seen him; 30″, he simply won't go away, she says; 10″, now he looks quite different, his nose is smaller and thinner, he has a helmet on his head, a determined looking man; looks like an ancient Roman; 1′ 20″, he is still there; 10″, I told her to shut her eyes; kept them closed five seconds; she saw nothing while they were closed, but the image was unchanged when she opened them again; 10″, he is gone at last; 35″, "There he comes again," said she;

"Oh, dear, dear, I wish he would go away;" 20″, he has turned into a bear; 10″, and now the bear has turned into a monkey; 10″, the image has entirely gone. All these images, she says, had the same eyes. Thirty seconds, "That dreadful looking man is coming back again"; 30″, no, it is another man, white robe, very large, smiling, costume like a monk's, not a Roman's; 30″, the head has faded out, the figure still there; when she moves her head it also seems to move. I put a black fountain pen between her eye and the object; it had no effect; 1′, all is gone except the milky or cloudy effect; 25″, a beautiful streak of blue appears; 10″, it is passing into a yellow; 10″, the colors are gone; 3″, now the color of heliotrope comes; 8″, it is becoming a rainbow, over the beauty of which she becomes enthusiastic; 24″, a window appears, a window-sill seat, interior view of a room, bookcases all around; 50″, it is gone; 20″, 'Oh, what is that trying to do?' 15″, 'I can't see what it is; the cloud moves into different shapes;' 45″, three dice appear; she sees the three and the five; cannot see the others. Stopped. Entire time, 23 minutes; no perceptible after-effect. Upon questioning her as to the source of the images, she said that she had been in Rome once, when about twelve years old. Had not been recently thinking or reading about kindred subjects. The old man, she thought, looked like one of Dickens' characters, but could not specify which one.

The only other case which I shall quote is that of Miss L., 18 years of age, an excitable girl, somewhat subject to slight hysterical attacks. Was in good health.

In three minutes 30″ sees a field, blue grass, stretching away off; gets pink by the horizon; there is a cloud on the grass, the cloud is getting pink; a face is coming out of the cloud; 1′ 25″, sees a figure to the face, gauzy drapery, pinkish near the cloud; cloud and drapery are the same; 35″, sky is blue; something or other is shining on the figure; cannot tell whether it is the sun or the moon; the hair of the figure is a bluish red, beautiful color; 30″, "The clouds are rolling, and the beautiful woman seems to be rolling on too; she is holding something which looks like two strings;" 7″, she thinks it is developing

into Guido's Aurora; 23″, sees one of the horses; there is only harness where the other horse should be; sees box of chariot but no wheels; 1′, there is something there which she thinks is a Cupid's head, but cannot see it clearly; 25″, the other horse is visible; the picture is complete except the 'Graces;' they are not there; the sun seems to be moving along; 50″, two or three Cupids appear; they are sitting on the chariot and elsewhere; 45″, the unfinished picture is turning very red, it is becoming a high brick wall; the luminous body, whether sun or moon, is still there, but the rest is gone except the wall; 20″, there seems to be a window in the wall; a beautiful girl is looking out; she has the same head and face that 'Aurora,' *i. e.*, Apollo, had; her hair is growing longer and longer; 1′ 5″, there seems to be a figure on the ground, it is a greyish-blue; 20″, seems to be playing on harp; sees only the head, arms and harp; the rest is cloudy; 25″, the clouds are becoming drapery; 10″, there are hills around and the sun is coming over the hills; 28″, the instrument seems to be something between a violin and a harp; 12″, the man has a red suit, a sixteenth century cape at back; 40″, half the field seems to be covered with her hair, which grows and grows; the man is wound up in it; 20″, the wall is gone; there seems to be only a sea of hair or water; 20″, the sun is still rising, it has been there all the time (probably a reflection); 40″, the hair is blue; it looks like the ocean; 30″, there seems to be something on the water; it looks like a shell; 30″, there seems to be a yellow head and a crown on the shell, eyes closed, the mouth opens; 45″, the hair comes again; it is red, short, crimped; the tongue sticks out; it is two yards long; 45″, there is a shore to the right; the tongue reaches to the shore, like a suspension bridge; the head slides along the tongue to the shore; 45″, the patient inadvertently took her eyes from the glass, and upon looking back the phantasm was entirely gone.

That auditory hallucinations in every way analogous to the phantasm of the glass may be produced by the application of a continuous but indeterminate stimulus to the organ is, of course, a familiar fact. Not long ago, for example, a

paranoiac was treated in the University Hospital who complained of a continuous hissing sound, which from time to time was transformed into abusive language. The noise was found to be due to a chronic inflammation of the inner ear· this had served as the sensory basis for the hallucination. A young woman, who has had several auditory hallucinations occurring apparently spontaneously, tells me that the sound of water running from a spigot always induces auditory hallucinations of precisely the same character as those above described in the case of vision, and they are often accompanied by appropriate visual pictures of the pseudo-hallucinatory order. Mr. Myers reports similar hallucinations produced by listening to the 'sound of the waves' in a large shell.

It is, however, in the phenomena of automatic writing that the most interesting illustrations of these principles are to be found. Automatic script is usually regarded as affording evidence of the existence of preformed, subconscious idea-systems which thus seek expression, and for a long time this was my own view. But I am now convinced that in many cases the writing is produced precisely as these sensory processes are produced—by the continuous application of an indeterminate stimulus to the highly organized writing mechanism. It is, then, essentially a purely motor phenomenon.

I was first led to this view by the study of a remarkably interesting case which was brought to my attention about a year ago. The patient, whom I shall call A. B., was an educated man, who had some knowledge of psychology and was acquainted with the conception of mental automatism. He was sitting one evening around a table with some friends, one of whom was supposed to be 'mediumistic,' to see whether rappings and levitation of the table could be had. Suddenly his left arm was drawn violently down; in a few moments the motor disturbance was transferred to the right arm. Pencil and paper were procured and the hand made desperate efforts to write. Much violence was displayed, the hand being pounded upon the table so furiously as to bruise the fingers and snap pencil after pencil. Nothing

legible was written. The case was brought to my notice in its early stages, and I had the opportunity of watching its later development. The violence at first displayed gradually disappeared. The hand learned to print and then to write a legible script, much resembling that of the patient. The content of the writing always professed to come from the patient's deceased friends—a claim which was readily disproved to the complete satisfaction of the patient himself. To him the hand seemed to be moved by some power not his own, yet he could at any time control it. The thoughts bore a marked resemblance to his own, but were not consciously furnished by him. He said it seemed to him as if he were watching another person write; often he would correctly anticipate the words to follow, but quite as often they would prove other than he had expected. At first B. himself believed that his dabbling in spiritism had started into activity a mass of subconscious idea-trains which had succeeded at the séance in bursting out into the motor mechanism. He was naturally not a little alarmed, and the further progress of his case did not tend to reassure him. The contracture of the right arm which always preceded the writing became easier to produce, and finally showed a tendency to appear spontaneously. At times he found it difficult to resist. Similar contractures appeared in the muscles of the left arm, of the legs, and finally of the face. By this time a general motor hysteria was produced. At the same time automatic sensory symptoms made their appearance in a manner that should delight the hearts of all who believe with Prof. Baldwin in the natural dependence of the sensory system upon the motor. Automatic idea-trains held converse with the patient at all times, seasonable and unseasonable; flashes of brilliant white light were occasionally seen while falling asleep, and the motor excitement rose to such a point that the patient escaped a hystero-epileptic convulsion only by two hours of strenuous resistance, followed by a timely dose of assafœtida. Throughout the patient believed himself to be struggling, like the possessed of old, with a secondary personality which was striving to overmaster his upper self.

This interpretation of the facts I believe to be entirely erroneous. They can be explained in a much more simple fashion. The original invasion might well have been a fatigue convulsion, due to the fact that the patient had for a long time held his hands outstretched upon the table. I have frequently seen contractures and convulsions produced under such circumstances, although not as violent as those of the patient are said to have been. The continuous but indeterminate stimulus applied to the centres—probably the subcortical centres of reinforcement and coördination—produce in time a reflex response. The neurologist who does it in order to study the phenomena gets purposeless contractions. The spiritist does it with the notion that the table is to shortly move under his hands, and the cortical processes which correspond to this expectation discharge downwards through the Rolandic region and impose upon the automatically produced contractions a semi-purposive character, producing lateral movements of the hands, which serve to move the table. I have seen this in more cases than one, and have proved to the satisfaction of the 'medium' that the movements of the table were due solely to the automatic contractions of his own arms. In the case of B., who had recently seen what purported to be spirit-writing, and had been much impressed thereby, the suggestion supplied by the cortical centres took a slightly different form and determined the otherwise meaningless movements to assume the form of writing. At first the patient allowed himself no expectation as to the content of the writing. But as he watched it scrawling away he naturally fell to wondering whether this was meant for such and such a word and that for another; he would ask whether it were, and then the word would be plainly written. Evidently, say I, in response to the suggestion given by him. As soon as he became accustomed to the thought that he might 'anticipate' the words his hand wrote, the process of genesis became easy. And it was made easier by a further suggestion which he gave himself. He asked the alleged spirit once when I was present by what means he communicated, and how it was that he, the patient, seemed to anticipate the spirit's thoughts. To which the writer replied that he supplied the thoughts himself tele-

pathically, it being the function of the 'medium' to write them down only! No wonder that the communications were thereafter much facilitated.

It was a comparatively easy matter to prove the true origin of these utterances as soon as our attention was drawn to it. We found that the hand never wrote anything intelligible when B. resolutely refused to attend to the writing. It was not necessary to look at it, as the motor sensation was sufficient to serve as a guide to what was being written, but it was necessary that B. should thus 'anticipate' it. If he did not the hand would write scrawls. He found also that the hand responded readily to all manner of suggestions. He had but to think, 'Why does it not try to print,' and forthwith it would print. He would ask the writer to bring a new spirit to write. It would flit through his mind that perhaps the new spirit would not be able to write. The new spirit would appear and write with labor and difficulty, or would print like a child. Then with 'practice' these characteristics would disappear and the newcomer would write as well as any *habitué* of B.'s organism. If while the hand was writing the illegible scrawls that always followed when B. did not help it out, he suggested, 'Why do you not try printing?' it would try printing, and produce page after page of curious symbols resembling some unknown language. I have no doubt that the 'unknown languages' often written by 'mediums' have a similar origin. On one occasion a 'new spirit' began writing a backhand. He announced that he was born in 1629, the figures having an archaic look. B. knew something of archaic script, and it occurred to him that the spirit wrote a hand suspiciously modern for one who was born in 1629 and died in 1685. Within a few lines the style changed to a twisted, gnarled hand, which certainly resembles a specimen of seventeenth century script with which I compared it. B. says that at the time of writing he thought the automatic script was written more rapidly than he could write archaic script of that character, but upon trying he found he could write identically the same hand voluntarily quite as readily.

Little by little the motor disturbance originally confined to the arm centres began, if my theory be correct, to involve

other centres. It first invaded the left arm centre, in response to the patient's suggestion, and then the legs and jaws. The development of the symptoms of ideal and sensory automatism I cannot analyze in detail. They were due doubtless to a progressive central disorganization of some kind, but I know no reason for it. Yet that the disturbance was primarily motor is proved by the fact that three or four days complete rest, with anti-spasmodic treatment, proved sufficient to put an end to it. B. still retained his power of producing the writing—a power which he rarely exercises, I believe—but the spontaneous symptoms entirely disappeared.

The teaching of such facts is plain. B. might well have become a classic case of 'secondary personality!' He presented many of the symptoms which are usually ascribed to subconscious idea trains, and I think I would myself, if he had not been an unusually good witness as to the subjective side of the phenomena, have put that construction upon them. As it is, this case, in conjunction with others of the same character, has thrown a flood of light upon the whole problem for me. I do not deny that mental states may exist subconsciously, and may be subconsciously integrated into complete dreams, and even into independent personalities. I would not wish to question, upon the strength of the four or five cases that I have studied, the conclusions of such careful investigators as Pierre Janet and Binet. Yet it is very difficult to correctly interpret the significance of these automatic phenomena in terms of consciousness when the patient is incapable of giving any clear account of their subjective feeling. We must remember that, while consciousness is revealed to us through the motor mechanism only, it does not necessarily follow that a given motor phenomenon always bears witness to the existence of the conscious state which it usually expresses. In these cases the significance of the motor phenomena was undoubtedly due to states in the upper consciousness, to which they at first glance seemed to bear no relation whatever, but it seems quite possible that apparently significant motor phenomena of a high degree of complexity may be produced by the subcortical mechanism without the coöperation of the cortex, and in all probability without any form of accompanying consciousness.

WELLESLEY COLLEGE PSYCHOLOGICAL STUDIES.

DIRECTED BY MARY WHITON CALKINS.

I. Dr. Jastrow on Community of Ideas of Men and Women.

BY CORDELIA C. NEVERS.

In an article called 'A Study in Mental Statistics,' which appeared in the December, 1891, number of the *New Review*, Prof. Joseph Jastrow discusses, among other subjects, "The Community of Ideas and Thought-Habits of Men and Women." His data are fifty lists of one hundred disconnected words, twenty-five from the men and twenty-five from the women of his Wisconsin University psychology class. His chief conclusions are "that women repeat one another's words more than men," and that "there is less variety among women than among men;" that the feminine traits revealed are "an attention to the immediate surroundings, to the finished product, to the ornamental, the individual, the concrete; while the masculine preference is for the more remote, the constructive, the useful, the general, the abstract."

For the purpose of testing these results the young women of the experimental psychology class at Wellesley College were asked to write out similar lists of one hundred disconnected words. That the thought-process might be as free as possible, no restriction was made. The students were not even asked, as in the case of Dr. Jastrow's class, to write as rapidly as possible, but this difference in the method cannot possibly be supposed to account for the wide difference in results. The subjects had no knowledge whatever of the purpose of the investigation. Twenty-five papers from those first completed were selected for the comparison; all were

rejected which were not declared by the writers to be the result of a natural flow of thought, and two were set aside because the words formed sentences.

According to Dr. Jastrow's first generalization, 'women repeat one another's words more than men.' He finds that "female students in their 2,500 words used only 1,123 different words, while their classmates used 1,375. Again with regard to classes, the women show different preferences from the men. The repetitions in the names of articles of apparel, of interior furnishings, predominate with women. Similarly the men have more repetitions in the names of animals, and mention more such names." The Wellesley lists tell a different story. Only fifteen lists, that is 1,500 words, have been studied from this point of view, but among the 1,500 are 1,103 different words, only 20 less than those among the 2,500 of the Wisconsin University women, and only 272 less than those among the 2,500 of the Wisconsin University men.

Further comparison is based upon Dr. Jastrow's division of his words into twenty-five classes. There may, of course, occur a deviation from Dr. Jastrow's principles of assignment of particular words to appropriate classes, but with reference to the classes which he especially emphasizes (words of wearing apparel, for instance, and abstract terms) there is no chance for difference of opinion. In the following tables of results Dr. Jastrow's order of frequency has been followed:

Comparative table of frequency of mention:

	Men of Wis. Uni.	Women of Wis. Uni.	Women of Wellesley.
1. Animal kingdom,	254	178	223
2. Wearing apparel and fabrics,	129	224	96
3. Proper names,	194	153	141
4. Verbs,	197	134	114
5. Implements and utensils,	169	121	132
6. Interior furnishings,	89	190	84
7. Adjectives,	177	102	234
8. Foods,	53	179	56

9. Vegetable kingdom, - - -	121	110	91
10. Abstract terms, - - - -	131	97	280
11. Buildings and building materials, -	105	117	106
12. Parts of body, - - - -	101	105	34
13. Miscellaneous, - - - -	91	99	162
14. Geographical words, - - -	97	80	142
15. Mineral kingdom, - - - -	74	96	54
16. Meteorological and astronomical, -	85	76	26
17. Stationery, - - - - - -	60	89	26
18. Occupations and callings, - -	71	47	33
19. Conveyances, . - - - -	62	52	79
20. Educational, - - . - - -	34	76	167
21. Other parts of speech, - - -	96	5	41
22. Arts, - - - - - - -	33	61	44
23. Amusements, - - - - -	30	53	102
24. Mercantile terms, - - - -	30	29	15
25. Kinship, - - - - - - -	117	32	18
	2500	2500	2500

The differences between these Wisconsin and Wellesley women's lists are very striking. According to Dr. Jastrow's results, the class to which women contribute most largely is that of 'articles of dress.' Of every 11 words, 1 belongs to this class, and the women have 224 such words as 1 against 129 on the men's list. Our records, on the other hand, swell the numbers of this class to only 96 words, giving it but 1 word in every 26. And if arranged in the Wellesley order of frequency this class would have been twelfth instead oi first.

In the sixth class, again, our results differ widely from those of Dr. Jastrow. He writes: "The prevalence of words denoting the common articles of furniture and interior fittings of a house—the peculiar field of woman's household instincts—is quite as marked, such objects being mentioned 190 times by women and 89 times by men." Our lists include only 84 such words, suggesting, perhaps, a lack of household instinct on the part of the Wellesley students, who appear less domestic than even the Wisconsin University men. In the class of 'foods' the disproportion of the Wisconsin record is even greater: the women mention words

of this class 179 times and the men only 53 times. The Wellesley records include, however, only 56 words of this sort.

In reference to words referring to amusements, arts and educational matters, our statistics corroborate the statement of Dr. Jastrow, that, in these subjects 'women show an excess over the men. "This," he observes, "points to a characteristic difference in the interests of the two sexes." The Wellesley students mention words referring to amusements with suggestive frequency—102 times.

"In the young men," Dr. Jastrow says, "we see marked preference for names of animals, of implements and utensils, the names of professions and similar relations." Up to this point our results agree, though the distinction which he notes is not in every case so marked. But the preponderance of abstract terms in the men's lists, which Dr. Jastrow accentuates strongly, vanishes utterly when these records are compared with those of the Wellesley students. Our lists contain 280 abstract terms, not only more than those (131) of Dr. Jastrow's men, but more than those (228) of all fifty of his students. In one paper alone occur fifty abstract words; the writer was specially questioned, and insisted that the progress of the thought had been entirely natural. If this unusual record be omitted, there still remains a number slightly greater than the Wisconsin total. It should be added that the Wellesley students earlier in the year had written several associated lists, involving almost necessarily the use of concrete terms, so that any artificial inclination to abstractness was unlikely. The prevalence both of abstract terms and of adjectives in the Wellesley lists is of questionable rhetorical import.

The conclusions from this study are thus chiefly negative, but not for that reason unimportant. The figures are too few to allow any positive deductions, but their uncompromising contradiction of Dr. Jastrow's results gives a needed warning concerning the dangers of such comparative study of the mental processes of men and women. If the serious study of the supposed psychic differences between the sexes can lead to conclusions so opposite to each other, the worth-

lessness of ordinary generalizations is made very clear—based as they are on purely personal observation and, usually, on the comparison of men and women of entirely different training.

II. PREVALENCE OF PARAMNESIA.

BY MARGARET B. SIMMONS.

An incidental corroboration of the ordinary opinion that paramnesia, in its various forms, is very common among normal people, was obtained in the course of some experiments on association[1] with the Wellesley subjects already mentioned, and with members of the Harvard Psychological Laboratory. A series of colors, each of them quickly followed by a numeral, was shown; and afterwards the colors alone were exposed, in succession, with the request that whatever numeral occurred to the mind of the subject, at the appearance of each color, should be written down. The main object of the experiment was to discover the number of times in which that numeral which had appeared in the first series in close connection with a color, would now be associated with the same color appearing alone. The subject was also asked, however, immediately after writing down a numeral, to record by the use of the signs +, — and ?, his opinion of the correctness of the association. These opinions form the material of the following summary of cases of a form of paramnesia, which may be called False Recognition.

Regarding first the totals, it will be observed that almost half (47%) of our entire number of recorded opinions are instances of paramnesia. The first vertical column of the table contains, as will be observed, the number of cases of what may be called negative paramnesia, not the persuasion that a mere object of imagination is a memory image, but the failure to recognize an actual reproduction. This distinction may best be studied by reference to the analysis of the phenomena of memory into two factors (objective and subjective), namely, recurrence or reproduction, and recog-

[1] *Cf.* PSYCHOLOGICAL REVIEW, Sept., 1894.

nition. These cases of 'negative paramnesia,' in which the correct numeral is judged to be wrong, involve actual recurrence, unaccompanied, however, by recognition; the instanstances in which the incorrect numeral is judged to be correct involve a failure of reproduction, as well as a lack of recognition. The fact that this latter class of pseudo-reminiscence contains 323 cases, or three times as many as the class of negative paramnesia, indicate that these factors, recurrence and recognition, strengthen each other or fail together.

FALSE RECOGNITION.

	The numeral written *actually is:*			Total.
	Correct.	Incorrect.	Half correct.	
Correct recognition, ...	*372* (76.54%)[1]	*104* (24.35%)	*75* (59.05%)	*551* (52.98%)
False recognition (paramnesia):				
Numeral, *judged* correct, ...	0	90	29	119
Numeral, *judged* incorrect,. . . .	36	0	23	59
Numeral, *judged* doubtful, . . .	78	233	0	311
Total paramnesia,	*114* (23.45%)	*323* (75.64%)	*52* (40.94%)	*489* (47.01%)
Total cases .	486	427	127	1,040

[1] The per cents are calculated on the totals at the foot of the vertical columns.

SENSORY STIMULATION BY ATTENTION.

BY PROFESSOR JOHN GRIER HIBBEN,

Princeton University.

The important function which is attributed to attention in the processes of sense-perception is very strikingly illustrated in an interesting instance which has recently been brought to my notice, and which throws into sharp relief the phenomena of attention operating in an intensified manner, and consequently modifying sensation to an extreme extent. It very often happens that normal tendencies are most clearly exhibited in unusual or abnormal cases, because there then occurs an inhibition of counter or complimentary tendencies, thus presenting to the observer the unique operation of an undisturbed and unmodified function. Instead of the resultant of many, there is the sole functioning of the one element separated from its usual concomitants. A force thus revealed in isolation is more readily appreciated whenever seen conjoined with accompanying forces in any system however complex.

The instance I wish to present seems to me to be of this nature, a case where the normal functioning of the attention is intensified in a very unusual degree. It is the case of a child about eight years old, a little girl, who as a baby was supposed to be congenitally deaf, as she gave no evidence of hearing any sounds whatsoever. Somewhat later in her development, however, it was noticed that at certain times she seemed to hear, and this always in connection with some objects of special interest, as bright pictures, toys, etc. And this now characterizes her general ability to hear: whenever the subject is one especially interesting to her, she hears without great difficulty; but whenever there is no interest in conversation, it is with greatest difficulty that she can be made to hear at all; and it is impossible to gain her

attention by any sounds, however loud, if she is engrossed in any absorbing task or play. Connected with this naturally there was an extremely tardy beginning of speech and very slow development, though during the last year—her eighth year—there has been a marked acceleration of her progress in this particular. In Preyer's classification of the imperfections and derangements of speech there is no precise place for such a case as this, as it is neither peripheral deafness, nor yet does it seem to be any cerebral derangement.[1] The difficulty seems to be psychical rather than physiological.

The facts as I have given them were narrated to me by a trained nurse, graduate of the New York Hospital Training School, who was in my family for a month or more during this last winter. She had been for several months in the family attending the mother of this child, and had had abundant opportunity for observation and for acquiring accurate information upon the subject. Moreover, she is a woman of very unusual ability, as one of the visiting surgeons of the New York Hospital assured me, and therefore one whose account can be wholly relied upon. In addition to her report of the case, however, I called upon a physician in New York who had known and observed this child for several years, and he fully corroborated the statement as made by the nurse in all particulars. I learned from him also that the child had been examined three different times by an eminent specialist in New York, and no defects either in the outer or middle ear could be detected. The indications all pointed to normal peripheral conditions, and the marked variations in hearing seem due to the central fluctuations of interest and the corresponding concentration or distraction of the attention. This appears only as an exaggerated form of the normal affect of interest upon attention. As Bradley[2] says: "If an idea engrosses, then any sensation which is connected with that idea may in consequence engross. And attention so far has appeared to consist in interest, either direct or transferred; an account which, we shall find, will

[1] *The Development of the Intellect*, p. 36 ff.

[2] *Mind*, Vol. XI, p. 310.

hold good everywhere" (*Cf.* Waitz, *Lehrbuch*, 634–7). In the ordinary phenomena of hearing, we recognize two momenta, the external stimulus and the inner adaptation of attention. According to Prof. James,[1] "the natural way of conceiving this is under the symbolic form of a brain-cell played upon from two directions. Whilst the object excites it from without, other brain-cells, or perhaps spiritual forces, arouse it from within. The latter influence is the 'adaptation of the attention.' The plenary energy of the brain-cell demands the coöperation of both factors. Not when merely present, but when both present and attended to, is the object fully perceived." Now, in sense-perception the two momenta, stimulus without and attention within, are normally so related that the former generally predominates, and is capable of arousing the activity of the latter, which is thus in a sense a function of the former, always reacting when the stimulus is sufficiently intense. In the case which we are considering, however, the attention does not function in an instinctive manner in response to an outer stimulus, and seems independent of its degree of intensity; but is readily aroused by the inner interest, and then alone is the consciousness of the outer stimulus rendered possible. This child, for instance, understands the sign language, and that is resorted to in order to communicate with her until interest has quickened the attention, and that in turn has stimulated the hearing. This is similar, in a much lessened degree, to the ordinary cases of what Herbart styles apperceptive attention—viz., where strained attention brings to consciousness external stimuli otherwise unnoticed. And this is similar also to what Prof. James[2] refers to as the 'ideational preparation' for sensation, in which, of course, there is increased attention, reinforced by the dominant idea present in the mind. The function of attention in sense-perception is illustrated by Wundt[3] with weak auditory stimuli, as the ticking of a watch at some distance from the ear, so that it can be perceived only with some strain of attention,

[1] James, *Psychology*, I, p. 441.
[2] *Psychology*, Vol. I, pp. 453, 439.
[3] Wundt, *Human and Animal Psychology*, pp. 256–7.

but will fall below the limen of consciousness without any relaxation. At intervals of three or four seconds the regularly recurring impressions alternately appear and disappear. In this child's case, however, the attention is not merely a factor necessary to discriminate concerning very weak stimuli, but the very strongest stimuli cannot excite the attention through reaction; it can only be centrally aroused; the hearing, then, being a function of the attention in her case, rather than the two being complementary functions determined by a law of action and reaction.

Moreover, a child's attention is characterized ordinarily by an extreme susceptibility to the sights and sounds of the outer world, and responds almost instinctively to sensorial stimuli. Sustained concentration of attention in childhood is unusual.[1] It is in mature age that attention follows our permanent interests, and only those objects associated with such interests find place in consciousness. Absorption in contemplation occurs only when a large group of associations have for years been forming about the controlling interest. And even then, with interests of greatest compelling power, diversion occurs whenever sensorial stimuli are sufficiently increased in intensity. This child, however, can have no developed associations of any considerable extent around her controlling interests; and yet her absorption in the same is complete, and her attention is incapable of being distracted. There is also a marked difference in her increased ability to hear whenever questioned concerning scenes which she has herself witnessed and in which she has taken part with evident pleasure to herself. For instance, after attending an exhibition of Hagenbeck's animals in New York, she heard and replied to all questions put to her concerning the animals and their performances. In this no doubt there was an ideational reinforcement of the auditory stimuli through the memory pictures still vividly impressed upon her mind. The ideational processes causing motor discharges which in time would increase the intensity of the sensation. This would form a stimulus additional to the mental energy arising from the increased interest already

[1] James, *Psychology*, I, p. 417.

mentioned.[1] In this connection it may be of interest to quote a sentence from Prof. James that bears upon this point: "We see how we can attend to a companion's voice in the midst of noises which pass unnoticed, though objectively much louder than the words we hear. Each word is *doubly* awakened; once from without by the lips of the talker, but already before that from within by the premonitory processes irradiating from the previous words, and by the dim arousal of all processes that are connected with the 'topic' of the talk. The irrelevant noises, on the other hand, are awakened only once. They form an unconnected train."[2]

In accounting for such a phenomenon, it is well also to take into consideration the physiological conditions which tend to increase the intensity of a sensation when the attention is unusually concentrated upon it. There seem to be indications of increased circulation in the parts concerned. This is stated by Dr. Cappie in his article on 'The Physiology of Attention and Volition'[3]: "The mental effect produced by an impression on a sensory surface is stronger, and details about the impressing cause are more completely gathered in when the mind is concentrated on it. . . . Two factors, at least, may be specified as bearing on this problem. In the first place, when the consciousness is engrossed by an immediate sensation, the sphere of encephalic activity is comparatively restricted. In the second place, the encephalic circulation will be focused in the direction of activity. The molecular agitation occasions a necessity and an attraction for more blood, and determination of this takes place all the more freely on account of the quiescence of the large part of the brain. The latter has, as it were, loosened its hold on the circulation, and the impetus towards those parts which have an attraction for it is thus all the stronger. The increased activity of the circulation then reacts on the energies of the tissue, and the mental effect produced is therefore greater."

[1] Baldwin, *Mental Development*, pp. 462–3.
[2] James, *Psychology*, Vol. I, p. 450.
[3] *Popular Science Monthly*, Vol. XXX, pp. 231–2.

Now, in the case of this child, it seems as though the conditions, both physiological and psychological, are present in exaggerated form, in order to produce the unusual results. The difference, however, between this case and normal instances is a difference rather of degree than of kind.

An additional feature of interest lies in the similarity between the phenomena we have been considering and the phenomena often accompanying attacks of hysteria. In such cases there is generally a very restricted field of attention, and the patient becomes so completely absorbed in some engrossing subject as to appear completely oblivious to all sensorial stimuli whatsoever. For instance, take such a case as cited by Pierre Janet: Lucie, while talking to one person, was insensible to all sounds about her, and could even be touched without being conscious of it. And when Léonie was knitting or writing, you might open or shut the door with a loud noise, speak to her, touch her, etc., without her perceiving it at all. Moreover, there were parts of her body which were so extremely sensitive to touch as to provoke cries of pain and even convulsions; and yet, when preoccupied by work or simple conversation, she could be touched upon the same hyperæsthesia spots, with no indication that she perceived this.[1] This account is very similar to the report which I received in reply to a letter which I wrote to the nurse above mentioned, making further inquiry concerning this child's case, and asking particularly whether she could hear when spoken to from behind, where her attention could not be aroused by any visual stimulus. The following is the answer which I received: "Her ability to hear when interested, in comparison with times when not interested, is very marked. She can hear if you stand behind her and talk very loud, but not very well; and *never* when she has her mind concentrated on another object; for instance, if she is at a window, looking out at something which has her attention, it is impossible to make her hear."

A case somewhat similar, yet with certain interesting peculiarities, is quoted by Pick from Pitres'[2] *Leçons cliniques*

[1] Pierre Janet, *L'Automatisme Psychologique*, pp. 188–9.
[2] *Zeit. für Psych.*, p. 168.

sur l'hysterie: "The patient, with eyes open, could hear; but with eyes closed could not; with one eye open it was possible to hear in the ear opposite, but not in the ear upon the same side as the opened eye." In this same article by Pick, *Ueber die sogenannte Conscience Musculaire* (*Duchenne*), there is a general historical survey from the time of Duchenne to the present concerning cases of anæsthesia, in which motor activities have been mediated through visual attention, indicating the quickening of motor as well as sensory functions, by means of a concentration of attention. In all cases where there was not this aid of attention through auditory or visual direction, the attempted movements were impossible. It has been also observed that in the somnambulistic state subjects are sensitive to the voice of the hypnotizer, but do not hear other voices. M. Janet mentions the case of one in this condition who could see a candle lighted by himself, but not those lighted by others; and adds that such "is not peculiar to the somnambulistic state, but exists in high degree among all persons susceptible to suggestion. It is an exaggerated state of distraction which is not merely temporary, and not the result of voluntary attention directed exclusively to one sense; but it is a state of natural and perpetual distraction which prevents these persons from perceiving any other sensation than that which actually occupies the whole field of consciousness."[1]

The case of this child seems to occupy a position midway between the temporary and permanent states of distraction as mentioned in M. Janet's account. In all of the similar instances which I have given, as parallel to this case, I have endeavored to indicate varying stages of concentrated attention from the normal to the exaggerated and abnormal; and as closely related phenomena we may consider them as different manifestations of one and the same tendency—the intensified force of attention producing an exaggerated modification of the intensity of sensorial stimuli.

[1] P. Janet, *L'Automatisme Psychologique*, p. 189.

SHORTER CONTRIBUTIONS.

PRACTICAL COMPUTATION OF THE MEDIAN.

BY E. W. SCRIPTURE,

Yale University.

In the PSYCHOLOGICAL REVIEW for Jan., 1894, I called attention to the practical and theoretical advantage of the median over the arithmetic mean. Since that time the median has been employed in many thousand adjustments with entire success. The theoretical justification has, however, been lacking, and correct formulas have not been given.

In the *Studies from the Yale Psychological Laboratory*, 1894, II, 5, I have presented a theoretical consideration of the median. Owing to condensation the practical computation has not been explained in such a way as to be available to the non-mathematical reader. Fechner is the only one who has attempted to give practical rules. They are not quite correct, and, as experience during the past year has shown, are incomplete. I propose here to put the matter in a practical shape for actual computation, all theoretical discussion being avoided. Suppose that you have a set of n measurements, $x_1, x_2, \ldots, x_n$. Starting with the smallest value, check them off in order of size till you come to the $\frac{n+1}{2}$th value. Then, as a verification, start with the largest value and check off downward to the $\frac{n+1}{2}$th value. For an odd number of measurements the two results will agree; for an even number they will agree or be adjacent in order of size.

For example, suppose we have the following results of a set of experiments on reaction-time: 213, 215, 214, 210, 212, 214, 215, 210, 212. There are 9 values, and the middle value is the $\left(\frac{9+1}{2}\right)$th or 5th. The smallest is 210, the next smallest is 210, the next 211, etc. The 5th is 213. The largest is 215, the next 215, etc. The 5th is 213.

Let us take, as another example, the set of values: 44, 51, 46, 50, 47, 49, 47, 45, 48, 50. The median will be the $\left(\frac{10+1}{2}\right)$th or 5½th value. The 5th from the smallest is 47; the 5th from the largest is 48; the 5½th will naturally lie between the two, and we take 47½ as the median.

A third variety of examples arises by taking a set of results where several values fall on the same number as median. Suppose we have 127, 123, 121, 123, 125, 124, 121, 120, 122, 123, 124, 123, 123, 123, 123, 122, 121, 125, 124, 121, 122.

Result, ...	120	121	122	123	124	125
Times of occurrence,	1	4	3	5	4	2

The $\left(\frac{19+1}{2}\right)$th or 10th value is 123. The expression $M=113$, however, does not mean $M=123{,}000\ldots$ but $123.5 > M > 122.5$, because we have been rounding off by errors of scale and errors of number all records to the unit-place instead of writing them to an indefinite number of decimals. Owing to the fact that 5 values fall upon 123, we know something about the first decimal place.

We are entitled to suppose that all values of 123 have arisen from values evenly distributed between 122.5 and 123.5. Counting from the smallest end, we have 8 values up to the limit of the region covered by the middle values; counting from the larger end, we have 6 values down to the other limit of this region.

The general formula is expressed as follows: Let r be the value on which the median falls, and let there be m such values. Let the number of values above r be a and below r be b. Then take $c=a-b$ and

$$M=r+\frac{c}{2m}.$$

This will give the decimal places for the median.

In the last example $b=8$, $a=6$, $c=-2$, $m=3$, $r=5$, and consequently

$$M=3+\frac{-2}{10}=2.8.$$

Although we know something about the first decimal place, we know nothing of any worth regarding the second decimal place unless $m > 100$.

To make clearer the third kind of example, I subjoin a set of specimens that show how the changes in c affect the value of M.

1.)

$$\begin{array}{ccc} & r = 13 & \\ \underbrace{11\ \ 12} & \underbrace{13\ \ 13\ \ 13\ \ 13} & \underbrace{14\ \ 15\ \ 15\ \ 15} \\ b = 2 & m = 4 & a = 4 \\ c = 2 & M = 13 + \tfrac{2}{8} = 13\tfrac{1}{4} & \end{array}$$

2.)

$$\begin{array}{ccc} & r = 13 & \\ \underbrace{11\ \ 12} & \underbrace{13\ \ 13\ \ 13\ \ 13} & \underbrace{14\ \ 15} \\ b = 2 & m = 4 & a = 2 \\ c = 0 & M = 13 + 0 = 13 & \end{array}$$

3.)

$$\begin{array}{ccc} & r = 13 & \\ \underbrace{10\ \ 11\ \ 12} & \underbrace{13\ \ 13\ \ 13\ \ 13} & \underbrace{14\ \ 15} \\ b = 3 & m = 4 & a = 2 \\ c = -1 & M = 13 - \tfrac{1}{8} = 12\tfrac{7}{8} & \end{array}$$

4.)

$$\begin{array}{ccc} & r = 13 & \\ \underbrace{9\ \ 10\ \ 11\ \ 12} & \underbrace{13\ \ 13\ \ 13\ \ 13} & \underbrace{14} \\ b = 4 & m = 4 & a = 1 \\ c = -3 & M = 13 - \tfrac{3}{8} = 12\tfrac{5}{8} & \end{array}$$

5.)

$$\begin{array}{cc} & r = 13 \\ \underbrace{9\ \ 10\ \ 11\ \ 12} & \underbrace{13\ \ 13\ \ 13\ \ 13} \\ b = 4 & m = 4 \\ c = -4 & M = 13 - \tfrac{4}{8} = 12\tfrac{1}{2}, \end{array}$$

6.)

$$\begin{array}{ccc} \underbrace{9\ \ 10\ \ 11\ \ 12} & \underbrace{13} & \underbrace{14\ \ 15\ \ 15\ \ 15} \\ b = 4 & m = 1 & a = 4 \\ c = 0 & M = 13 & \end{array}$$

The computation in the last two kinds of examples is so

simple that it can be performed with great rapidity. Those of the other kind are almost as easy as soon as the computer becomes familiar with the median. Indeed, the economy of the median, in respect to time is something that can be appreciated only by some one who is obliged to compute many sets of results.

THE SECOND YEAR AT THE YALE LABORATORY.

BY E. W. SCRIPTURE,

Yale University.

Probably the most important event during the year was the introduction of the median as a mean value in place of the arithmetical mean. Early in the year it became evident that the arithmetical mean was a poor one to use for the results obtained and the median was substituted. This value is the middle one from either extreme in a set of values arranged in the order of size. It had been theoretically discussed by Laplace and Fechner, but had never been put to practical use. The main reason for this radical departure can be summed up briefly as follows: 1. Psychological and statistical measurements almost always follow an assymmetrical law of probability, and for such cases the median is a better representative than the arithmetical mean; 2. the saving in labor of compensation is very great; 3. the small numerical disadvantage is almost never of importance. The first article of the second issue of the *Studies from the Yale Psychological Laboratory* makes an attempt at a thorough theoretical treatment of the median and a comparison of it with the arithmetical mean. The article, although crude and incomplete, may derive some value from the fact that the author subjected it to criticism and correction by several mathematicians, notably Prof. Gibbs.

The most extensive investigation of the year was by Mr. Gilbert on the mental and physical development of school children. About 1,400 children were subjected to tests and measurements on the muscle-sense, color-sensitiveness, suggestibility, reaction-time, discrimination-time, time-memory,

voluntary motor ability and fatigue, weight, height and lung-capacity. The ages are from 6 to 17. Tables and curves of results show the laws of development with age.

The work is presented in the thesis in so condensed a form that it is impossible to further summarize it. Its great value probably lies in the opening-up for investigation the field of mental life in school-children by the invention of suitable methods and apparatus. It is a contribution to child-study and anthropology, in which the methods of the new psychology are applied for the first time. The use of mean variation, of two kinds for the study of the children is a notable innovation.

An investigation on the highest audible tone led to the following conclusions: 1. The pitch of the highest audible tone varies directly and almost proportionately with the intensity; 2. the highest audible tone is much higher when proceeding from silence to sound than when proceeding from sound to silence; 3. fatigue has apparently no effect; 4. fluctuations were found similar to the fluctuations for the threshold of intensity; 5. above the highest audible tone there is still found an indefinite, somewhat painful sensation.

Experiments on the education of muscular control and power lead to the conclusions: 1. Steadiness of movement can be increased by practice; 2. this increase in steadiness is not limited to the muscles immediately trained, it affects the control of the corresponding muscles on the opposite side of the body; 3. the training consists principally in training in steadiness of attention; 4. the increase in muscular power by training, as measured by a dynamometer, is partly transferred to the opposite side without training.

Some experiments made on fencers and others seem to justify the statements:

1. The average fencer is not quicker in simple reaction (where a few mental elements are involved) than a trained scientist.

2. When once the mind is made up to execute a movement, fencers are far quicker in the actual execution.

3. As the mental process becomes more complicated, the

time required by the average fencer is greater than that required by a trained scientist.

4. The general conclusion seems to be that fencing does not develop mental quickness more than scientific pursuits, but it does develop to a high degree the rapidity of executing movements.

The investigations mentioned thus far are published in the *Yale Studies.*

Throughout the year the investigation of hallucination and suggestion has been continued; it will be brought to a close in the coming year.

An investigation on the least perceptible change in the intensity of light has been carried on at intervals during two years, but is still left unfinished.

The scientific policy of the laboratory remains unchanged, the main effort being to train the members to accurate and reliable work. That this is not always successful is generally due to previous defective mathematical training.

In the account of the laboratory for the first year (PSYCHOLOGICAL REVIEW for Jan., 1894), mention was made of the success of the small workshop. During the past year the equipment was largely increased by the introduction of a two horse-power motor, shafting, etc., and by the purchase of new tools. It is the desire of the laboratory to offer facilities to all psychological institutions for the construction of fine apparatus.

The officers of the laboratory during the year were: E. W. Scripture, instructor and director, and C. B. Bliss, lecturer and assistant. Mr. J. J. Hogan was employed as instrument maker and electrician. Dr. Bliss left at the end of the year to take the place of assistant professor in the University of the City of New York.

DISCUSSION.

THE NEW PSYCHOLOGY IN UNDERGRADUATE WORK.

The rapid development of laboratories for the study of Physiological and Experimental Psychology has silenced the chief objections to such methods of investigating the facts of mind. It is doubtful if any other subject ever experienced as rapid changes in method and point of view as has psychology during the last five years. We need not go back fifteen years to find the beginning of the first laboratory in America. The first was also the only one till 1887. During the next three years about one laboratory per year was begun in this country; then several came at once, and for three years thereafter they fairly poured upon us. The past year has witnessed the establishment of fewer well-equipped laboratories, probably because all the more important schools of our land had already inaugurated the work. The universities of Michigan, California and Minnesota, in which psychology has always been subordinated to History of Philosophy and Ethics, have not attempted to withstand the tide. For the past two years the chief progress, perhaps, has been in the normal schools and in modifying the methods of teachers of philosophy in the smaller colleges. The text-books of men who are believed fairly to represent the attitude of Harvard, Yale and Princeton toward the 'materialism' of the new movement, have done more than all else to make easy the growth of modern psychology in our denominational colleges and in our normal schools.

Now that a firm foothold is acquired, may we not profitably look about us to discover, if possible, where we stand? The establishment of more than thirty psychological laboratories in this country in the last five years was not the result of a carefully matured plan to revolutionize instruction in philosophy. Indeed there has been no planning done for the college world at large, and unfortunately, perhaps, very little for any institution in particular. The men and women at the head of these laboratories have come from several independent schools for original investigation, or from one or two schools in this country with much the same object, or have picked up their knowledge by reading and private experimenting. Probably not one of these directors has followed any general plan in use

elsewhere. We have, therefore, as many ideals as we have individual directors. Let nothing be said unfavorable to such a condition. It has doubtless made stronger men and has been a prominent factor in the extraordinary success of the new movement. Even we, however, may learn from the experience of others, and our successors must depend largely upon our work.

It does not seem out of place to inquire into some of these ideals with a view to determine the nature of their agreements and the extent of their differences. If sufficient interest in this subject exist, a discussion of the aims and possibilities of the new psychology may lead to a considerable extension of its usefulness without necessarily interfering with the individuality of the various laboratories and their already efficient directors. It is with a hope to provoke such a discussion that this short paper is written.

In the first place, we note that the new psychology has not taken the place of the old psychology, in the sense of having been substituted for the latter in the required course of study. While nearly all colleges formerly required psychology for graduation, at least in some of the courses, there are very few (I know of none) that require physiological and experimental psychology for graduation in any course. The extension of the elective system has been a most potent means of introducing the new work. In what different ways this is true need not be enumerated, as nearly every school has made the change within the last few years.

Most of the older universities were content to require a certain amount of 'philosophy,' usually a smaller amount, than formerly; the student was thus at liberty to select his specific work with the consent of the professor in charge. In many of the newer universities, especially in the Central States, philosophy was eliminated entirely from the required work in some or all of the numerous courses leading to the bachelor's degree.

We may now see some effects of these steps when taken in conjunction with the following facts: First, in the larger and older schools several professors are engaged in teaching the different subjects generically called philosophy. As a rule these men are scholars of experience, power, skill, influence, and reputation. Also, as a rule, the new instructor in experimental psychology has been a recent graduate lacking some of the above-mentioned qualities. It is very evident why it has seemed inadvisable to attempt to develop an undergraduate course that would attract the general student. Hence, in nearly all large universities of the East, experimental psychology is considered a post-graduate, or at least an advanced,

philosophical discipline. Students are admitted to its work under various conditions, but generally after work in other lines of philosophy. If we all feel now as I felt when I first determined to study the new psychology, this plan would be quite rational—indeed conscience would demand it; the intellectual, moral and religious safety of the students would require such a preparation against the evil temptations therein. There is, of course, no question of conscience in this method. It is merely the result of conditions that are difficult to remove, and perhaps, in most cases, whose removal is not desired.

The additions to this young science must come mostly from men whose professional duties are light and whose energy is commensurate with their leisure. These professors and their well-trained students furnish valuable contributions every year. The animating spirit, the successful progress and the new recruits must always come from men whose entire time is given to research or to students engaged in research. The post-graduate work in psychology is even more necessary than such work in other departments. The following lines, therefore, will not be understood as an attack upon post-graduate work in psychology, nor even as an underestimation of the value of such work.

Is there a field for experimental psychology in undergraduate work? The writer knows there is sometimes in some places, and will endeavor to present a few points that seem to him to indicate a wider field for such work than, according to his personal knowledge, is now covered.

After the foregoing assertions it will not be surprising to learn that the facts in support of my present thesis are furnished mainly by a few Western universities where laboratories for undergraduates have been successfully conducted with classes, largely elective, whose numbers form as large a proportion of the entire student body as do the classes in any branch of philosophy in other institutions. It does not follow, however, that the conditions of success in these lines are peculiar to the West, though it is probably owing to the greater relative importance attaching to the instructor or professor of the new psychology. In small schools there is seldom another with whom to share responsibility or authority. The sole professor of philosophy may conduct his work as the chief executive and the faculty will endure. These external conditions are more favorable in the small school. I shall not attempt to show that in large schools the corresponding less favorable conditions can be improved. It is the internal evidence that must now engage our attention.

It is not sufficient to show that large classes in physiological and experimental psychology are organized every year in several Western schools. This fact shows the possibility of doing such work with undergraduates and with overworked instructors. It remains to show that the contrary practice of many schools arises chiefly from the circumstances already alluded to, or at least that the neglect of the new psychology in undergraduate work is not due to any constitutional defect in the methods of this science. The apparent difficulties are not insuperable. The most formidable one is doubtless the need of a laboratory. But a laboratory is also necessary for advanced work. A valid objection, however, is discovered in the time required for this work. Better supervision is required than for laboratory work in either chemistry or physics. This demands personal attention from the instructor in psychology. I think this objection is unanswerable. If instructors in psychology are unwilling to do this kind of work, we must wait until another species of instructor can be evolved.

It ought to be unnecessary to describe the effect on the student, of a laboratory course in psychology, and yet, like chemistry and physics and botany and zoölogy, this new science will have to fight for every inch of ground. The battle will probably take much the same general course. The first campaign has been won, and psychology may now have a laboratory for advanced workers. It is not my purpose to show that it is worth while to begin the study of all science by direct appeal to the original sources, nor shall I attempt to answer that valuable proposition concerning the necessity of book learning. We certainly cannot obtain all knowledge from our personal experience. It is generally recognized, however, as good policy to have some original ideas or personal experience before we depend much on books. There can be no defence of the introduction of the new psychology into beginning classes without practical work. Logic and metaphysics and the dictionary may be well taught without a laboratory; physiological and experimental psychology require some *things* to see and *feel*. Unless the spirit of the new method is breathed into the work, logic, metaphysics or ancient psychology may as well retain their place as introductions to philosophy. This spirit cannot be communicated to a large proportion of students by the most brilliant lecturer with a text-book as assistant. If it can be imparted in any manner, by any means, at any cost, it is worth serious consideration. It is said that only one student in ten receives much benefit from the required course in philosophy, and that students seldom elect additional work in this

line outside of lecture 'snaps.' Philosophy at least should be taught philosophically. No one denies that the most elementary principles of method are ignored by very many college teachers. Philosophy, perhaps more than any other discipline, is a sufferer from this cause. The self-activity of the individual is talked about in a more or less interesting manner, but there is often little successful effort made to start this activity a-going.

The writer does not believe, with some college professors, that philosophy is good only for the few. The introductory course in philosophy that does not force half the class into activity, and stimulate them to further systematic study, fails in its mission. Interest is of course essential; but interest that does not lead to inquiry, the interest that disappears with the professor's manuscript, is mere emotional debauchery.

Let us try to analyze the causes of the success of the new psychology as an introduction to the study of philosophy. Retaining our concrete form of exposition, we have first to note the kind of mental food the beginner in philosophy has been accustomed to. He is perhaps a junior. In every instance at least half of his previous study has been in language. Nearly half the remainder in mathematics. The other fourth has been given to science or to language, or in a few schools it may have been given to history. The first years in science do not always have the philosophical interest that might be attached to them if instructors were not bent on making specialists. Too often, though needless, the mechanical accumulation of facts, the unaided rediscovery of simple truths, 'discipline' blinds for a time the learner. In nearly all cases he is not led to connect his new knowledge with himself. This is not a fault of science, but is the misfortune of some teachers of science.

Language teaching is recognized as especially prone to dwell on the form and to neglect the substance of literature. It sometimes becomes painfully inhuman if not fiendish in its neglect of heart and soul. History is certainly alive. It is, moreover, at least anthropoid. The first years, however, are usually devoted to times having least in common with our own. It is difficult for the young student to put himself in place of the common citizen of Peiræus or Ostia. The generalizations of history are especially difficult because of the complexity of conditions and the social and political inexperience of the learner.

The junior, therefore, comes to psychology with considerable mental discipline, with some formal knowledge of the world's history, and with more or less information concerning isolated facts of several sciences.

A general course of physiological and experimental psychology with laboratory practice ought not only to add to his store of isolated facts, or even merely to impart additional mental culture; the opportunity for philosophical instruction must not be lost. The needed facts of the associated sciences will be brought together; their relations will become clear, and gradually there will grow up a rational appreciation of the interdependence of the forces of nature. Everything now points to man and to me. Its humanity is, of course, the chief characteristic of philosophy. The new psychology would fit the beginner to understand that all the universe is akin. This is the first opportunity the general student will have for bringing together many results of his previous study. Psychology is better fitted than any other branch of philosophy to suggest and to direct this reorganization, because it furnishes very many of the necessary facts, and does not plunge the inquirer too suddenly into bottomless speculation. I do not think it desirable or possible to *exclude* metaphysics from this introductory course, but the material is very largely positive, and the transition may be made so gradually that students will naturally *grow* the wings of generalization.

Besides furnishing essential materials for the further study of philosophy, this introductory course should familiarize the student with the characteristics of philosophic thinking, and, above all, should test his taste for work of a similar nature. If treated as an *introduction to philosophy*, and not as a course in philologic encyclopedism, or as special preparation for specializing in a specific specialty, the first year in the new psychology ought to stimulate *many* students to further work in related lines.

It therefore should be recommended to juniors and should occupy about one-fourth of their time for a year. At least two hours per week should be spent in the laboratory. The class work should not be formal and impersonal lectures, nor should it be time-killing oral quizes. Informal conferences, mutual quizes and explanations should be accompanied by frequent *written* reviews. If possible the *instructor* should be suppressed and the director and inspirer brought into his place. Such a course affords sufficient opportunity to become acquainted with the work of at least half a dozen modern writers besides a score of classic monographs. Yet, with the laboratory work, this is more than the introductory course in philosophy usually accomplishes.

H. K. WOLFE.

UNIVERSITY OF NEBRASKA.

A REJOINDER.

I regret very much that my review of Dr. Hodgson's address should seem to him unfair or lacking in courtesy. In his reply he is good enough to say that what he regards as a misstatement of his reasoning on my part was of course unintentional. Had I to rewrite the review I should with equal courtesy state that Dr. Hodgson's securing his conclusions by 'blowing both hot and cold in his premises' was unintentional, but at the time I wrote this fact seemed to me so obvious as to be undeserving of mention. Dr. Hodgson set out with a determination to make 'truth the paramount consideration.' He has, however, as I cannot but believe, reasoned badly; and after having carefully read his reply and re-read his address, I fail to see that I have done him injustice. If he dislikes the expression of which I made use in describing his reasoning, I very willingly withdraw it, for it was no part of my purpose to cause needless pain to a writer whose works I read with both pleasure and profit. To me the phrase seems a harmless one, and much less energetic than the expressions of which he has himself made use in commenting upon my review.

As to the three confusions attributed to me: the careful reader of page 99 of the January number will notice that I nowhere confuse Mr. Hodgson's 'material world' with his 'universe.' That part of the universe beyond the limits of the known world of matter I call the 'world of faith.' Mr. Hodgson has himself called it an 'unseen world' (pp. 16, 17, 18), and has stated that belief in it is 'a state of mind which we call Faith' (p. 18). In the part of my review to which Mr. Hodgson objects it is treated precisely as it is in the part which he finds appreciative and generous, and for which he thanks me. Of the nature of the 'material element' which fills time and space in this world of faith I have said nothing.

The careful reader will notice, in the second place, that I nowhere confound Mr. Hodgson's 'material element in consciousness' with his 'matter' or 'material world.' Indeed, when I use the word 'matter' to indicate the material element in consciousness, I put it in quotation marks, and when I use it in the ordinary sense, as in the phrase 'the world of matter,' I do not do this. At one period of my life (with some shame do I confess it) I was a thorough Kantian, and I spent too many years in the company of 'matter' and 'form' to be capable of so gross a blunder. I am quite sure that an attentive reading of what I have written will reveal that I have not made this blunder.

As regards the third confusion—that between Mr. Hodgson's 'perceptually known' and his 'positively known'—here we have the very 'Kern' of the 'Pudel;' and in his use of these terms lies, as I believe, the very head and front of Mr. Hodgson's offending. I saw, of course, in reading his address, that he did not make the two strictly synonymous, and I have nowhere indicated that he did. I saw, however, with equal clearness, that to make them synonymous when treating of the 'formal' element in consciousness, and to distinguish between them when treating of the 'material' was an inconsistency which one might not unjustly characterize as 'blowing both hot and cold.' That Mr. Hodgson has been guilty of this inconsistency I can best show by giving a brief sketch of his argument.

He begins (Section II) by analyzing consciousness into its material and formal elements, concluding, as a result of this analysis, that infinity must attach, in the last resort, to time and space alone (p. 6). Section III is taken up with a digression on the nature of intensive quantity. In Section IV he returns to what he declares to be the main question of the evening, the conception of infinity, and sets before himself the double task: (1) of ascertaining its meaning, 'and consequently the nature of time and space in respect to infinity;' and (2) of discovering 'to what other things the attribute of infinity may be found to attach, in virtue of their connection with time and space, and what views of the Sum of Things are imposed upon us in consequence of the relations so disclosed' (p. 8). He concludes here that 'space and time in their entirety are wholly limitless and inexhaustible' (p. 11), since it is impossible to perceive or imagine a limit to or in space or time without perceiving or imagining it as having space or time on both sides of it.

Section V points out at length that the infinity of time and space are 'facts of perception,' not conceptions. 'We observe them,' he declares, 'alike in the minima and maxima of perception' (p. 11); 'what thought does with them is to make them objects of conception, out of, and as well as, objects of perception' (p. 12). A conception is declared to be a man-made entity in contrast to a perception independent of conceptual thought (p. 13); the conception of infinity is our gathering up of the facts constituting our perception of infinity into a single conspectus (p. 14). Thus Mr. Hodgson makes the infinity of time and space facts of perception—perceived facts. We do not infer or assume that time and space are infinite, but we perceive it, if, indeed, the above statements and others like them mean anything at all. Surely I am justified in thinking that Mr. Hodgson admits perceived facts to be worthy of

the name of positive knowledge, and that time and space 'perceptually' known as infinite are 'positively' known as infinite. The passage I shall quote just below will make clear that he does not regard the infinity of time and space as an article of faith.

This brings me to the concluding section of the address, which discusses the view of the universe involved in taking 'the perceptual facts constituting infinity as a disclosure, as far as they go, of its true nature.' It takes up, in other words, the second part of the task which, in Section IV, Mr. Hodgson set before himself. He reasons (p. 15) as follows: "Next, then, let us * * * * * * see what follows from that view of infinity which I have endeavored to lay before you this evening, infinity as depending on perceptual facts. Since on this view the Universe is known to us only as existing in time and space, and these are known to us as extending beyond any boundary which we can conceive or imagine; and since, moreover, time and space are known to us, each in its kind and province, as inseparable co-elements of existing things, it follows that we must conceive existence as extending commensurably with time and space, beyond the boundaries of existence as positively known or imaginable by us. For in the infinity of time and space is involved their existence beyond the limits of any content positively known to us, and in their existence is involved that of some co-element or other, though not positively known to us, seeing that it is only as a co-element that we know them. You see how important are the two facts, disclosed only by analysis, first the illimitability of the formal element in consciousness, secondly its inseparability from its material co-element."

From the above I think Mr. Hodgson's mode of procedure in getting his world of faith is clear enough, as is also the defect in his reasoning. He finds in consciousness two inseparable elements: one of them is illimitable (p. 16), perceived to be illimitable (p. 11 ff.), *known* to be illimitable (p. 15). Since the two are inseparable, he argues, the other must be illimitable too, though we do not positively know it. In my review I pointed out that our positive knowledge ought to extend just as far in the case of the one element as in that of the other. Are they not inseparable? And if the infinity of space be a perceived fact, must not the infinity of that which fills the space perceived (I do not say *matter*) be a perceived fact too? Space and time, says Mr. Hodgson, cannot be perceived alone. Surely consistency would require Mr. Hodgson to maintain that the universe is *perceived* to be infinite, and that, if there be a world of faith, it can at least not be assumed as an indispensable

co-element to fill empty space and time. According to Section II of the address there can be no gap to fill—'this occupation of what I will call *the formal element* by feeling is essential to consciousness.' Were there, by any possibility, an empty space or time to fill, Mr. Hodgson's argument would lapse from the mere fact that the material element would thereby be proved not to be an indispensable co-element, and inseparable from the formal.

In his reply Mr. Hodgson admits that, when we perceive space and time to be infinite, 'some kind of material element in consciousness' shares their infinity. This admission is fatal to the argument. We have here a co-element already given, and none need be assumed. Is space perceived to be infinite? then so is this. Mr. Hodgson evidently thinks that such a co-element will not serve the purpose, and faith must provide another. His reason for this I think I can guess. His 'perceptual knowledge' does not really mean perception, although he has (Section V) treated it as such. When I perceive a chair, the content is as unmistakably perceived as the form. My knowledge of either element is equally positive. Can we in the same sense say that we perceive space and time to be infinite? Are we talking of genuine perception? Do we really *perceive* either the space and time or the material co-element that shares their infinity? Surely not. 'Perceptual knowledge' of the infinity of space and time I believe to be a delusion and a snare, and I believe it would conduce to clearness to avoid the expression altogether. It does not concern the question of the consistency of Mr. Hodgson's argument to enter into a discussion of the infinity and infinite divisibility of space and time. For this reason I omitted the discussion in my review of the address, and for the same reason I omit it here, merely referring anyone who may feel a curiosity upon this point to what I have printed elsewhere.[1] That perceptual knowledge does not really mean perception, even to Mr. Hodgson, appears to me evident from the language of his reply. He defines a 'perceptual knowledge' of time and space as a *representation* of them as filled with some material element in consciousness; and in what precedes he uses, as though they might, in his argument, be made to stand for each other, the expressions 'perceived,' 'represented in thought' and 'thought of as existing.' The inconsistency of Mr. Hodgson's reasoning lies, as I have already pointed out, in the fact that his 'perceptional knowledge' is made equivalent to perception and gives positive information in the case of the formal element in consciousness, but

[1] 'The Conception of the Infinite,' Phila., 1887. 'On Sameness and Identity,' Phila., 1890, § 36. 'The Philosophy of Spinoza,' 2d Edition. N. Y., 1894, p. 274, ff.

has a different meaning and fails to give such information in the case of the material element. The inconsistency stares one in the face.

In the last section of his address Mr. Hodgson has inserted two arguments for his world of faith, which form no part of his main discussion, and might very well have been advanced by one who had no sympathy with his ideas on infinity. The one is, that the world of matter does not furnish answers to all the questions it raises, and that these answers may be found in a world beyond it. The other is that there may well be modes of existence of which our present senses can give us no information. Upon these arguments it is unnecessary for me to dwell, for they have, as I have said, no necessary connection with the 'infinity and co-element' argument which I have criticized. G. S. F.

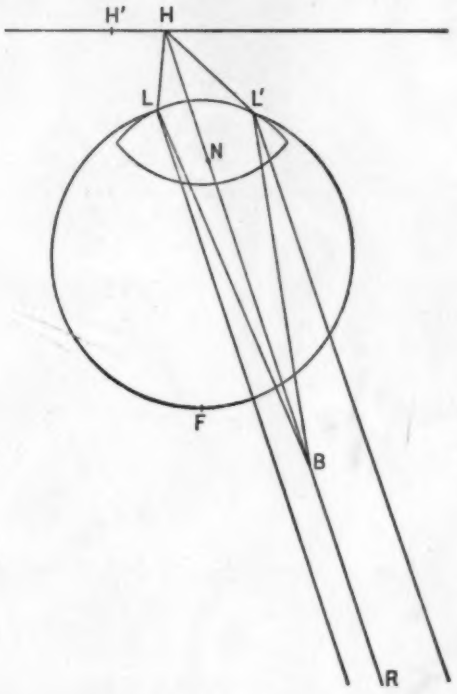
PHILADELPHIA.

SHADOWS OF BLOOD-VESSELS UPON THE RETINA.

If light is admitted into the eye through two small holes in diaphragm held in the front focal plane, two images of a blood-vessel in the front part of the retina are cast upon the sensitive layer. If now red and blue light are admitted together through the hole, the two shadows cast by the red light are farther apart than those cast by the blue light; and Prof. König has accounted for this fact by supposing that the layer of the retina which is sensitive to red rays is farther behind the blood vessel than that which is sensitive to blue rays, and, in fact, is behind the rods and comes altogether and in the pigment epithelium. Schapringer[1] says that the fact can be accounted for more simply. His argument is this: The point at which rays proceeding from a source of light are brought together is in the line joining the source of light with the nodal point of the eye (HN in the figure), and all the rays which reach this point are within a cone whose apex is this point and whose base is the pupil of the eye. For a source of light near the eye, this point is behind the retina, and farther behind for red rays than for blue. Now, the apparent position of the source of light is determined by the centre of its diffusion-circle, and the centre of the diffusion-circle will be farther away from the axis of the eye than is the point in which the line HN meets the retina, and the more so

[1] *Findet die 'Perception der verschiedenen Farben in ein und derselbe Lage der Netzhaut statt?* Pfl. Archiv., LX, 296–302.

the less refrangible are the rays of light. The virtual position of the image of H will, therefore, be farther from the axis if H is a red object than if it is a blue object; and the more so, again, the farther the bright point H is from the axis of the eye—the line FN produced. What follows from this, with perfect rigor, is that two *objects*, H and H′, will seem to be farther apart in red light than in blue. Dr. Schapringer takes it for granted that this is the same thing as to say that the *shadows* cast by a minute object in front of the nervous layer of the retina will be farther apart in red light than



in blue. He says: "Hence in red light the shadow falls on a spot farther from the axis than in blue, and this is the reason why with two holes the shadows are farther apart in red light than in blue." This is, in the first place, very inaccurately expressed; the red shadows might both be farther from the axis than the blue shadows, and yet not be farther apart from each other; what is necessary is that the shadow from the point H′ be *more* farther away than from the point H. But overlooking this point (which is a mere slip of the pen), Dr. Schapringer's argument seems to involve a substitution of dissimilars. The position of the shadow has nothing to do

with the centre of the diffusion-circle, but is determined solely by the direction of that ray of light which has been intercepted by the blood-vessel. When the point H is in the front focal plane of the eye for red, the red beam of light is a cylinder, and the blue beam of light is slightly convergent. (The convergence is exaggerated in the diagram.) This convergence, Schapringer says, would be so slight that it may be neglected, and he accordingly makes the blue beam of light also a cylinder, but *proceeding in a different direction* from the red beam. It would seem that in doing this he throws away as insignificant the very difference in refrangibility in which the cause of the phenomenon must be sought, and at the same time assumes a difference in direction of the two beams which does not exist if they are both taken to be cylinders. Even if the beam of red light be taken to be a cone, then the case would be different according as the distance apart of the two holes, H and H′, is such that the shadow-casting blood-vessel is in the right-hand half of each beam, the left-hand half of each, or in the right-hand half of one and the left-hand half of the other; the red shadows would be farther apart than the blue or nearer together, according as one or the other of these conditions prevailed, or they might even happen to be equally distant. And it would still remain to be shown that this source of difference, if in the right direction, was sufficient in amount. Prof. König has as yet published only a brief sketch of his work, and the exact details of it are consequently not yet known.[1] It is, of course, impossible to suppose that he has not himself taken account of the difference of refrangibility of red and blue light. At the same time the reasons against his conclusion are so strong (this REVIEW, II., p. 144) that any simple means of accounting for the fact which would hold water would be very welcome.

C. L. FRANKLIN.

A COMMUNICATION.

Certain points emphasized in Prof. Armstrong's kindly notice of my book, *Philosophy of Mind*, enable me to make two or three desirable explanations.

In the first place, it is regarded as a defect, which increases he difficulty of understanding fully some of my positions, that the theory of knowledge postulated "is used as a kind of universal,

[1] When measurements are made the red and blue lights are used separately, but the accommodation of the eye would seem to be kept unchanged from the fact that the other eye is engaged in reading distances upon a scale.

with little or no systematic determination of subordinate criteria." This statement is partially true. But the answer to it, as a charge against my method or my conclusions, might be given in these very commonplace words: No one can say everything at once, and everyone has a certain right to follow his own notions of the best or the most convenient order in the discussion of connected topics on philosophy. In treating of the metaphysics of mind, I have simply postulated a general noëtical principle—which Prof. Armstrong himself admits is '*in some sort*' true for all those who believe in the possibility of knowledge as implicating reality—and have then at once proceeded to inquire, *what* do we know of that particular reality we call 'the mind,' and of its actual relations to the body and to external nature? Of course, those who take the position of the consistent positivist (if any such there be), or the deniers of all ontological science whatever, cannot accept my argument. For them there is no such possibility as a philosophy of mind, properly so called. But the 'systematic determination' of this vague general premise in noëtics is itself the complete theory of knowledge. Therefore, if at all by me, this defect of the *Philosophy of Mind* will have to be supplied later and in a separate treatise. I hope then to make it clear that the 'intermingling of the psychology and the noëtics of self-consciousness,' in discussing the metaphysics of mind, is no defect of any author's treatment, but an unavoidable result of the very nature of the subject treated.

Again, Prof. Armstrong finds my metaphysics 'familiar almost at a glance.' He declares it to be 'distinctly Lotzean in tone.' Now, the measure of truth in this declaration depends upon how far we agree in understanding Lotze and upon how Prof. Armstrong understands me. As for myself, after considerable study, I confess I cannot understand Lotze on many most important points; and on many others, where I think I do understand him, I cannot agree. But metaphysics, in the general sense of the term, is another subject on which I have as yet publicly espoused and incorporated into this particular metaphysical treatise, only certain general principles, to which every one who has any metaphysics whatever will agree, I think, when he does something more than merely 'glance at' my words. If the latest critic of Lotze interprets him correctly in the following sentence, then I certainly am not distinctly Lotzean.

According to Prof. Jones' conception of Lotze's views, "Ideality and reality are handed over respectively to the thought and to its object; so that thought is ideal *only*, and objects are real *only*, or thought is ideal without being real, and its objects are real without

being ideal." Or, again: "Just as the real world takes no part in the thinking process, so the results of that process, the conceptions, classifications, judgments and influences, are not copies of reality, nor do they in any way represent really existing facts or events." It would be difficult to gather into two sentences of the same length views more unlike my own, as to the nature of both thought and reality, than these Lotzean utterances—if, indeed, they are to be so characterized.

In general, may I not utter a mild—and, I presume, it will turn out ineffective—but earnest protest against being '*classed*' in any such fashion? Without pretence of independence or of originality, and with cheerful readiness to acknowledge and avail myself of the results of all successful research and sound thinking on the part of others, I much prefer not to be named after any one. The very motto I have chosen for this book is my protest. In this connection I will improve the opportunity to say that those critics who have spoken of my *Elements of Physiological Psychology* as modeled after Wundt, etc., simply go contrary to the facts. When I began my work in physiological and experimental psychology, now about sixteen years ago, I had never read Wundt's work (then only some four years old); I am not sure now that I even knew of its existence. The *Grundzüge*, and all the other work of the Leipzig laboratory, have always been used by me as any other work—making allowance for differences in quality, quantity and range. But I suppose that some of the younger men, who have been trained in the laboratories which have sprung up since I began to investigate, have difficulty in conceiving how any one could get at any truth in other ways than those to which they have been accustomed, or could venture an opinion within the mystical and exclusive circle of their own specialties.

I will speak of only one thing more in explanation. Prof. Armstrong complains, not bitterly but mildly, of my polemical style in certain chapters of my last book; and he rebukes me for making use of 'horrible examples.' On this point I will only say that, in order to discuss three or four views which I considered mistaken, I selected the most distinguished and respected examples of these views; and if in treating them polemically I was in any degree unjust, or in any place discourteous, I desire once for all humbly to apologize. Contrary to my usual custom, I adopted deliberately the polemical method as best adapted for discussing certain topics in this 'technical treatise.' My judgment as to the wisdom of this course I do not now withdraw. In general, I suppose there are

very few writers on psychology and philosophy who have used the polemical method so sparingly as I have; and, on the contrary, taking my theological writings also into the account, there are very few writers who have suffered so much from it, or so unjustly, at the hands of others, and in every way.

In general, too, I sincerely hope that the subject to which I am devoted may soon pass from the stage of pseudo-critical characterization to the stage of really critical and thorough discussion. Just now I should particularly like to have my destructive discussion of the science and metaphysics of the principle of psycho-physical parallelism discussed in its turn. I really think this principle needs sorely at present the assistance and tender nursing care of its champions. If a challenge is urgently needed to call forth these champions, I am sure the challenge should be issued.

GEORGE TRUMBULL LADD.

A NOTICE.

Rev. W. W. Campbell, the joint author of Study A., against whom Prof. Münsterberg in his 'Notice' fears I have personal emnity, permits me to state that "he has read my discussion of the 'Motor Power of Ideas' and finds it a dignified and sober criticism, without show of enmity, and such as any student ought to accept of his work."

HERBERT NICHOLS.

PSYCHOLOGICAL LITERATURE.

GENERAL.

Comte, Mill and Spencer: an Outline of Philosophy. J. WATSON. Glasgow, Maclehose; New York, Macmillan. 1895. Pp. vii+302.

The title of this book is a little misleading. Comte, Mill and Spencer are treated neither exhaustively nor comparatively nor on their own account nor exclusively; two names have at least an equal claim to figure in the title: Darwin, to whom nearly fifty pages are given—almost as much as respectively to Spencer and to Mill, and more than twice the space allotted to Comte—and Kant, the discussion of whose theories of morality, religion and art takes up fully a third of the volume. The aim of the book, as indicated by the sub-title, is to develop, in connection with the criticism of these five thinkers, a system of what the author designates as 'Intellectual Idealism.' For the main outlines of his system he rightly acknowledges his large and manifest indebtedness to Green and Edward Caird. Indeed, his book may be characterised as a systematic exposition in brief compass and in clear and intelligible language of the dominant British Neo-Kantian or Neo-Hegelian type of thinking. The method and the matter of exposition are both conformable to the traditions of the school.

The gist of the argument for the doctrine advocated is found in the chapter criticising Spencer's doctrine of consciousness. Spencer maintains that the very nature of consciousness forbids any transcendence of the distinction of subject and object; we are referred for the explanation of either to its opposite, but neither can be resolved into the other. The reply acknowledges the distinction, but seeks to show that the object is ultimately identical with the subject. The demonstration is as follows: No 'thing' is an isolated individual. All its determinations are expressions of relations to something else. The particular 'thing,' therefore, is a mode of existence as a whole. The object as a whole is, therefore, a systemated unity. Similarly of the organism: its bodily structure is related to the whole just like anything else. But its functions are inseparable from its structure. The total object is, therefore, not merely a systematic but an organic

unity. But the same considerations apply to feeling, in which the life of the organism and, therefore, of the whole world pulsates, and to the consciousness which feeling, furnishing its entire content, makes possible. The object is, therefore, not only a systematic and organic unity, but self-conscious intelligence or God (173).

The argument seems inconclusive. Granted that no individual—no bit of matter, no organism, no sentient or conscious being—is isolated, but that each is an aspect and mirror of the whole; granted, further, that man most perfectly mirrors the whole, inwardising in feeling, in a way, the whole of nature and conscious of this whole, in a degree, as he interprets the content of his feeling: does it, *therefore*, follow that the object is identical with the subject, and that the whole is one self-conscious intelligence? Hardly. It follows that the universe is of a sort to be mirrored in consciousness, and that it somehow includes the principle of consciousness in its nature; but it is a great step to the conclusion that the universe is itself self-conscious. This raises the whole question of the self. But neither at this point nor elsewhere in the book are the difficulties connected with this question rigorously dealt with. Professor Watson argues, apparently, that because each living being, each sentient being, each conscious being is in determinate connection with the whole, there is therefore a universal life, a universal sentiency, a universal consciousness. And because the existence of each preceding mode is a condition for the existence of the following, he concludes, apparently, that the lower is the possibility of the higher in the sense that it is essentially of the same nature. These conclusions may all be true, but they need more explicit mediation. The short and easy proof of Idealism is not enough. Nevertheless, as an outline of philosophy, it is doubtful if there is any other book in English that could be so cordially commended to young students.

SMITH COLLEGE. H. N. GARDINER.

An Introduction to Comparative Psychology. C. L. MORGAN. London, Walter Scott; imported by Chas. Scribner's Sons, New York, 1894. Pp. xiv+382.

Mr. Morgan's 'Introduction to Comparative Psychology' is an attempt to clear the ground for a science that has suffered thus far from the homocentric character of psychical analysis as comparative anatomy and physiology have suffered earlier from the almost exclusive interest that centered in the human organism.

With this end in view Mr. Morgan starts with the postulate of a monism. He is not, however, convinced of its necessity as a basis

for comparative psychology, and warns, in a note, all those to whom philosophic speculation has no attractions to pass over his prolegomena.

It is true that his subsequent analysis may be followed out from a dualistic standpoint. It is, for his practical analysis, a matter of indifference to Mr. Morgan whether physical and psychical be regarded as two aspects of the same entity or whether we conceive them as simply running parallel with each other. His thought is dominated by the analogy of the two aspects of the curve. It is certainly a matter of indifference to one who would study these two aspects, as respects their directions and mutual relations, whether he sees them as two aspects of one line or as the corresponding aspects of two lines that run parallel with each other.

It is in terms of just such tangible analogies that Mr. Morgan does most of his thinking. The whole treatise is built on three of them: (a) that already named between the two aspects of a curve and the parallel physical and psychical phenomena, (b) the analogy between a wave in an undulating medium and consciousness, and (c) that between our recognition of consciousness in another being and the inference a clock might make by means of the relation between its hands and its own inner works to the inner works of another clock from the position of its hands. These analogies cannot lay claim to any great novelty; their value lies in their simplicity and lucidity.

Such analogies, however, are at best makeshifts. They inevitably carry with them much error because they are analogies, *i. e.*, concepts which, for the sake of concreteness, are but incompletely analyzed and abstracted. We are very definitely of the opinion that psychology, especially comparative psychology, needs, on the contrary, a thoroughgoing analysis of its fundamental concepts to put it upon its feet.

Experimental psychology fares, to be sure, about as well with the incomplete analysis as it would with the most searching, because the most that such an analysis could do would be to justify it in its use of the methods of physics and physiology. The theoretical stage in no science has done much more than justify and free the methods which had been worked out in its first period of discovery. Experimental psychology would profit greatly by such a freeing of its tools, *i. e.*, in the definite formulation of a psychological method as ultimately distinct from those of the physical and biological sciences. Still it suffers as yet no serious set-back through this lack of definiteness in its own territory. A capital error, such as is involved in 'Fechner's law,' will hardly be committed again.

But the case stands quite differently with comparative psychology. The experimental psychologist has the test of immediate experience for the distinction which he makes between the physical and the psychical. The reality of the distinction is justified by the success of the life processes that assume it. The experimenter has therefore only to follow rigidly the essential reactions that make up his life and he need not go astray. This is but another statement for the assertion that the distinction between the physical and the psychical is an immediate datum of experience. And one must go a step further than this; the distinction between the physical and the psychical in others is as really an immediate datum of experience. We are as essentially social beings as physical and physiological beings, despite the analogy of the clocks. (We must deprecate the reappearance of this spook of Paley's watch, after its having been laid in the field of natural theology, to haunt the domains of a modern science.) The development of the distinction between the physical and psychical in others proceeds *pari passu* with that in the child's consciousness of himself—if for no other reason because he could never form the conception of himself as psychical without the conception of others. Or again man is essentially social.

The experimenter therefore runs no more risk of making an unreal distinction between the physical and psychical in others than he does in his analysis of his own consciousness. But just in proportion as our analysis leaves the stage of self-consciousness within which we live, and approaches those points of civilization where we are no longer perfectly at home, and especially when we leave the human intelligence quite behind and strive to reconstruct the consciousness of the lower animals, are we at the mercy of dangerous analogies which were before harmless. These analogies, in their proper place, serve as illustrations, that is act as stimuli to reconstruct what in all its details is fully within our power. The analogy of the clocks may serve fairly well to recall to one the process by which he revises his social judgment—detects, as it were, a social hallucination. It is as far from copying the state of consciousness in which a dog recognizes a hostile intruder, as the click of a calculating machine would be from describing his angry growl at the loss of half his dinner.

The same criticism holds in regard to the analogy of the wave of consciousness. This serves excellently to recall the onward sweep of concentrated attention through the mass of details that crowd the field of consciousness, and the positions of relative

importance which those details hold. But while this illustration summons up the reality in my consciousness, it is but the most superficial analysis of attention. This has the unity and direction of the purposive act, not that of a wave propelled by a *vis a tergo*. So that at the point where we are forced to abandon the concrete reality of our own full reconstructions, the analogy becomes as false as the reconstruction of Greek life by the romancer of the Middle Ages. The Socratic task of substituting analytical definitions for illustrations is that which faces the comparative psychologist. In the opinion of the reviewer this can only be successfully met when the logical process which is the reality of the distinction between the physical and psychical has been recognized.

UNIVERSITY OF CHICAGO. GEO. H. MEAD.

L'Année Psychologique. Ie. année (1894). H. BEAUNIS and A. BINET, 1895. Paris, Alcan, 1895. Pp. VII + 619.

The value of this excellent publication will be evident at a glance. Besides the contributions made during the past year from the Laboratory of the Sorbonne—reported in full, and deserving later notice—it contains the fullest and most adequate report of the facilities, equipment of the universities, the details of courses of instruction given in the United States that has ever been published. This excellent paper is from the pen of Prof. E. B. Delabarre. The curious error of the translator—M. Binet himself—in rendering the letters of the degree 'Ph.D.' by Docteur en Médicine, is corrected by a note on p. 619. The final part of the book, devoted to a bibliography of the literature of psychology, neurology, etc., for the year 1894, seems to be full and accurate. It comprises 1217 titles: but quite a large number of these are from the literature of '93, and it is difficult to see sufficient reason for their presence. Compared with the recent *Index* of Warren and Farrand, it runs short about 100 titles by number, but seems to include more supplementary headings put in as sub-titles 'a,' 'b,' etc., throughout. It has more French titles, as one would expect, and fewer English, than the American publication. One very grave defect, however, of the French bibliography, in the opinion of the present reviewer, is that all the titles are translated into French. The main function of such a bibliography, one would think, is that of giving to those who consult it exact details of the title, so that it can be ordered from the booksellers or referred to under its title in libraries. But how can one write for a book and be sure his order will be understood if what he orders is only the equivalent in another language of the real

title? I myself have actually had this difficulty, or rather uncertainty, in ordering a title from this *Année*. Suppose a Frenchman ordering Ladd's 'Manuel de Psychologie,' (an imaginary case) which of the author's three books would he be likely to get? French readers, on the other hand, are not much benefitted by this preference for French: for if a man cannot read an English or German title he is not likely to look up the book all of which is in one of those languages. The remaining portion of the *Année* is an extended collection of notices of important pieces of work issued during the year, and seems to have been done with great care and labor. Of course opinions will differ whether the most important things under each head have been selected; but that is unavoidable where selections are made at all. On the whole, the compilers of the *Année* are to be congratulated and thanked. It is pleasant, also, to note their announcement that the publication is to continue annually.

J. M. B.

Lehrbuch der Psychologie. W. VOLKMANN. 4te. Auf. Ed. by C. S. Cornelius. 2 vols. Cöthen, Schulze, 1894–5. Pp. VII + 511, V + 568.

All psychologists will welcome the fourth edition of the great work of Volkmann. It has long been a standard work, and although always partial to the Herbartian wing in its citations of literature no less than in the theoretical positions of the author, yet it has had great value as a work of reference. It must be said, however, that the chief value of the work in the edition before us is to be found in the 'real essence'—the original work of Volkmann—rather than in what is added to it by the 'accidents' of editing. The prospectus leads one to believe that the citations of literature, which have always been an important feature of the work, are well up to date and very full. Anyone looking for anything of the kind will be very much disappointed. The additions made to the literature at the end of the chapters are marked with an asterisk, and they turn out to be relatively meagre in the extreme. Having an eye on the works in English added to the bibliographies, the inadequacy becomes extraordinary. Looking under Emotion, I find no reference to James, nor to the literature which his chapter has called forth. Similarly, there is no reference to that author in connection with the *Innervationsgefühl*. Indeed, the only reference I have come across—I have not looked over every page, however, with this in view—to James' 'Principles' is the citation of the title of the book in the register of literature at the end. Similarly a search for the

literature of the writers in England shows the defect about equally marked, Ward, Sidgwick, and others being largely overlooked. This seems to me to be simply the old case of 'nothing worth knowing outside of my own country,' and it is astonishing that the large sale of earlier editions of the work of Volkmann—if no other consideration availed—did not lead Prof. Cornelius to have the foreign purchaser somewhat in mind. J. M. B.

Introduction to the Theory of Science and Metaphysics. A. RIEHL. Translated by A. FAIRBANKS. London, Kegan Paul, Trench, Trübner & Co., 1894. Pp. VIII + 346.

It would be hard to find a book in the more recent Kant literature of Germany whose translation could be more generally welcome to English and American students than Prof. Riehl's 'Science and Metaphysics.' The German original of the work forms the conclusion of a larger work, 'Der philosophische Kriticismus und seine Bedeutung für die positive Wissenschaft,'[1] which seems not to be generally known in this country or England. The work, however, has been influential in Germany, and this translation of its concluding part into English is sure to increase its usefulness materially. Moreover, Prof. Riehl, unlike many German philosophers whose writings have been translated recently, can congratulate himself that his book has been rendered into readable English. It should be said, however, that Prof. Riehl by his own admirable style, by his unfailing clearness and precision, has done more to assist translation than some of his compatriots. The German original of the entire work was reviewed with greatest care some time ago[2] by Prof. Adamson, who paid high tribute to the author's 'completeness of knowledge and maturity of philosophical reflection,' and recognized the great value of his statement of Kantianism.

This translation comes at a time when interest in the relation of science and metaphysics is reviving, or rather when the relation is getting a positive character. Metaphysical philosophy and science were not always apart. "The separation," says Prof. Riehl (p. 12), "of philosophy and science which has resulted in the regard of them as antitheses dates back no further than the period which in Germany followed Kant. . . . . This antithesis of philosophy and science forms only an isolated episode in the history of thought, which to-day appears to be more than a passing phenomenon only because it is so near us in time, and which is to be explained from

[1] Leipzig, W. Engelmann, 1887.

[2] Mind, vol. 14, Jan., 1889, pp. 66 ff.

special causes, namely, the excess of æsthetic culture over scientific among the German people." The excess of æsthetic culture helps to explain the distinction between phenomena and things-in-themselves, a distinction which in its turn is the explanation of the exclusion of metaphysics from science. The things-in-themselves or noumena, removed by Kant so absolutely from the sphere of the proper activity of the (scientific) reason, became at once the object of purely philosophical or non-scientific inquiry. They were necessary to Kant's system, but it was unfortunate for his criticism, doomed to subsequent shocks in history, that he could not be quiet about them; to call attention to them was fatal. Kant, then, denied the right of enquiry to metaphysics; he reduced metaphysics to a body of presuppositions; the inquiring reason could turn only to natural science; but his criticism rather stimulated than controlled the reason. The first influence of his philosophy was to make both philosophy and science extreme, each after its kind, metaphysics rising in its flights to all appearances far above the supporting atmosphere of experience, and science becoming ever more forgetful of its metaphysical presuppositions. Recent tendencies of thought, however, show science actually becoming interested in its presuppositions and philosophy returning to something like its old character of 'natural philosophy.' But the change only justifies Prof. Riehl's declaration of the isolated character of the episode or the passing character of the phenomenon which Kant's philosophy reports, and one has to wonder that he who saw so clearly could remain so loyal to Kantianism. Yet was he altogether loyal?

In regard to Riehl's Kantianism, Prof. Adamson says in the review already referred to that the work of Riehl, although in the treatment of its subject "characterized by so much freshness and originality of conception, so comprehensive an insight into the relations of philosophical and scientific problems, and so close a reference to the general tenor of modern science, as to render it in no sense a mere re-presentation of the work already achieved by Kant," is still no real departure from Kant; 'the form of expression is different,' but the difference 'involves no matter of great philosophical significance,' Critical Realism, only another name for Kantianism, being quite as hostile to metaphysical inquiry. But I cannot feel that this is altogether fair to Riehl. We may have to style him a Kantian,—some people depend on epithets,—but there are tendencies in Riehl's book that are rather treacherous to the Critical Philosophy. Thus the Critical Realism rests upon a theory of the immediate perception of the external world, and this involves

a change not to be overlooked of the Kantian conception of scientific knowledge.

Riehl's idea of science is truer, I should say, to psychology, with its final dependence on introspection, than to ordinary physical or objective science; so that, if we put two and two together, he seems to say that the modern 'natural philosophy,' which has arisen or is arising with the passing of the antithesis between philosophy and science, has a psychological point of view, if not also a psychological content. This may be interpreting Riehl to himself as well as to others, but present tendencies in philosophy make the interpretation attractive. For through psychology, especially through experimental and comparative psychology, more than through any other lines of investigation, science has been brought to a consciousness of its presuppositions, or at least to a consciousness of the fact that it has presuppositions, and this consciousness is entering into scientific experience as an all-important factor. Prof. Ladd's recent book, 'Philosophy of Mind,' is at least in its ideal true to the tendency here indicated. It shows how science, in spite of Kant or Kantianism, was doomed to become metaphysically self-conscious as soon as it should be well within the sphere of psychology. A sentence or two from Morgan's 'Introduction to Comparative Psychology,' a book of perhaps more value for the air that it breathes than for the words that it speaks, may be quoted here: 'I do not think that the metaphysics of the subject can be avoided in any such inquiry [as that of 'mental evolution in all its aspects.'] It is not a question of metaphysics or no metaphysics, but of good metaphysics or bad.' Metaphysics was once the original sin of the human reason, whose salvation could come only through the grace of a purely non-metaphysical science, but in these days original sin is a doctrine, not a fact, and a doctrine as untenable in psychology as in religion.

In conclusion let me call attention to the most suggestive turn that Prof. Riehl gives to Kant's *a priori* with its criteria of universality and necessity. He gives to the *a priori* a social function in experience. He would substitute for the explanation of knowledge from the point of view of the individual its explanation through society. "I am inclined," he says (p. 66), "to believe that with every perception by man is associated the impulse to communicate it. Experience is a social concept, not a concept of individual psychology." The bearing of this upon the psychology of language is evident. It also brings psychology and sociology to what is almost if not quite an identity. It points the way to relating the psychology

of Kant at once individualistic and rationalistic to the psychology of to-day with its double interest in 'ontogenesis' and 'phylogenesis.' Its special meaning for Riehl appears in later chapters, notably in the very valuable chapter on 'Determinism of the Will,' Part II, ch. III.

A. H. LLOYD.

UNIVERSITY OF MICHIGAN.

SOCIAL PSYCHOLOGY.

(1.) *La Sociologie: ses conditions d'existence, son importance scientifique et philosophique.* M. BERNÈS. Revue de Métaphysique, Mars, 1895.

(2.) *Sur la méthode de la sociologie.* M. BERNÈS. Revue Philos., Mars and Avril, 1895.

In (1) M. Bernès presents an eminently judicious and philosophical view of the object and method of sociology, and in (2) he applies his principles to a criticism of Durkheim's articles noticed in the REVIEW for May. The two dominant characteristics of contemporary sociology are declared to be (a), imitation of natural science (especially biology), and (b) absolute opposition to subjective (psychological) sociology under all its forms, in particular the refusal to attribute any *rôle* to the reflective will of the members of society. These two characteristics are sources of fundamental errors. To make sociology purely 'objective' is to deprive it of its essential character; and just in proportion as we make it abstract, objective, mathematical, are we in danger of leaving out the distinguishing mark of intelligent action. To give any meaning to economic, legal or political institutions of the past, we have to think ourselves into the state of society which they express, to interpret them by internal psychological causes. The common fault of both subjective and objective sociology is to identify the subjective with the individual; whereas by looking within we may pass the bounds of individuality as truly as by looking without. The society of which we form a part is within us as truly as we are in it. Sociability, more or less conscious, is as truly an element in the social reality of the present as is any objective social phenomenon. On the other hand, there is no ideal but in relation to a reality already existing; the existing solidarity is the occasion for the subjective practical or moral appreciation of things or actions which in turn becomes an important factor in future development. For society is a 'becoming' as well as a thing. Every collective aspiration which by its realization results in consolidating the group, in making it at once more complex, more

plastic, more conscious of itself, becomes thereby a cause of progress. In proportion as society is better distinguished from every other reality and corresponds better to its definition,—in proportion, that is, as it creates itself—it becomes more completely an object of science; but of a science whose laws are at once objective and ideal, expressing relations between what is already in existence and that which, while not yet existing, has already begun to be.

The most important addition made by (2) to the conceptions above noted is that of the psychological and social significance of action as relating the individual and the social. On the one hand, every state of consciousness is already an action, at least by anticipation; action is a principle of expansion which sets us in some fashion outside ourselves, and would cease to be action if we could make it entirely individual and internal. Thus by action the psychological and subjective life takes on already a social value. But, on the other hand, action is not wholly comprehended within the subjective idea of an internal principle of activity; it exists only by virtue of a sum of objective conditions which give it a body and a form, and without which it would be reduced to a mere potentiality—that is, to an abstract and fictitious entity. Action is thus the most elementary datum of social psychology. It is the bond between subjective and objective, between actual and ideal. It is the concrete fact from which these opposing terms are derived.

UNIVERSITY OF CHICAGO. J. H. TUFTS.

CRIMINOLOGY.

Naturgeschichte des Verbrechers. Grundzüge der criminellen Anthropologie und Criminal-psychologie. H. KURELLA. Stuttgart, Enke, 1894. Pp. 284.

Dr. Kurella, the author of this 'Science of the Criminal,' is the medical director of the insane asylum at Brieg, in Silesia, where he has had good opportunities for the study of criminals. During ten years he has carefully examined many hundreds of cases of insane criminals. He has also carefully studied the somewhat abundant literature on criminology that has been published during the last few years. As a result of his observations and reading he has become deeply impressed by the theories of Professor Lombroso of Turin, and in his introduction declares his enthusiastic adherence to the theory advanced by Lombroso, that there is a certain peculiar and distinct type of irreclaimable criminal, described as *delinquente nato*, who is, so to speak, fatally predetermined to crime, being the genuine born criminal.

The author thinks that sufficient observations have been made and results published to form the basis of a work giving the fundamental principles of criminal anthropology and criminal psychology. He believes that such a work will be specially useful to doctors consulted about criminals, to psychiatrists, judges, and all deeply concerned with the problem of the treatment of criminals. He therefore proceeds to give a summary of the chief results compiled by many recent writers on criminology. He specially mentions Prof. Lombroso, Turin; Prof. E. V. Hofman, Vienna; Dr. Sommer, Allenberg; Prof. Benedict, Vienna; Dr. Mingazinni, Rome; Prof. Dr. v. Tschisch, Dorpat; Dr. J. Karlowicz, Warsaw; Dr. Wesnic, Belgrade. In his register of authors consulted, we find some 162 names, some of whom have been cited as frequently as thirty-five times. A work of 284 pages, giving an epitome of the tabulated results of so many writers, cannot be summarized in a brief review.

To show the need of a more extensive study of criminals, he gives a mass of statistics to prove that crime is greatly increasing. It occurs to one in reading these statistics to ask, Are these conclusive evidence of the increase of crime, or of increased vigilance in the detection and punishment of crime? Do we mean by 'criminal' those who are arrested for crime? And the suspicion arises that statistics are being used uncritically, and that the examination of the meaning of 'criminal' is neglected. We think this distrust will increase as the reader proceeds.

I. *Criminal Anthropology.*—The author first treats of anatomical peculiarities of criminals. Diagrams and explanations are given of methods of craniometry. Tables showing characteristics of criminals in the general shape of the skull and structure of particular parts. Exact measurements corroborate the common suspicion of the low and retreating forehead. A list is given of marks of degeneracy or atavism frequently found. A detailed reference is given to structural varieties of the following: the ear, breast, sexual organs, beard, hair, excess in number of fingers, etc.; arrested development of organs; acquired characters, *e. g.*, tattooing.

II. Under 'biological factors,' he discussed nourishment and digestion, susceptibility to feeling, motor characteristics. Under 'Heredity' he gives interesting tables of criminal families through several generations (or degenerations!), showing the interconnection of nervous diseases, pauperism, alcoholism, insanity, prostitution, suicide, etc. The condensation of the book may be judged from the fact that, after referring to Dugdale's well-known 'The Jukes' as 'the most valuable contribution to criminal heredity,'

its results are presented in little more than half a page. He next discusses the criminal 'milieu.' Then gives a 'physiognomy of criminals,' with illustrations that remind one of Lavater. In nearly every case we notice smallness of head, or at least smallness of the fore part of the head. Frequently we see the following: Large under jaw and high cheek-bones; prognathic, platycephalic, skaphocephalic, parietal and occipital regions of skull large with small frontal region. Deformities of ears and lips. One particular form of ear is regarded as peculiarly indicative of the criminal. It seems that Mozart had this 'fatal ear.' His aberration from crime to music is explained by saying that this is not the only example of a close affinity between genius and degeneration; adding that the same excessive development of the sense of hearing, indicated by the extra development of the external ear muscle, is common to the musical genius and the burglar who partakes in the plundering instincts of the carnivora with the necessary accompaniment of acute hearing.

III. *Criminal Psychology.*—The author first discusses the theory of 'moral insanity.' He prefers Lombroso's assumption of the born criminal—*delinquente nato*—having the following peculiarities:

(1.) Parasitic tendencies very pronounced. (2.) Honor and truthfulness, regarded by the author as late products of civilization, are utterly lacking in the criminal who reverts to an earlier type. (3.) An interesting account is given of the traditions, codes and vagabond-slang-language of criminals.

IV. Under moral concepts and passions, he says: There is no thought of the future in the criminal; he acts in accordance with the passion of the moment; consequently all attempts to prevent crime by threats or by making an example of convicted offenders are futile. The criminal utterly lacks sympathy and pity. He is reckless, cruel, lazy; despises work as beneath his dignity, prides himself on being a criminal, craves notoriety, and desires to become a 'virtuoso' in crime. The author outlines a psychology of Ethics of Criminology, and quite significantly accepts Mr. Herbert Spencer and Lombroso as his authorities. Everything must be explained by feelings. A sentence will indicate the author's standpoint: "Now, the most superficial observation of children—and no one has shown this more clearly than Lombroso—will make evident that pity and the feeling of right are in the first place acquired, and in the congenital lack of these feelings the criminal is like all children" (p. 250). It is a pity the author did not make even a superficial examinatisn of children instead of giving us the dicta of Lombroso.

Would not a very little observation and *reflection* make evident that the child, not being self-conscious, is neither positively selfish nor positively unselfish, but simply *non-self-ish.* Surely the instinctive actions of the child would be less incorrectly regarded as social than as aggressively anti-social—which is the meaning of lack of pity in the case of the criminal. We confess that we are deeply disappointed with the chapter on Criminal Psychology. There seems to be evidence of a very slight acquaintance with the general psychology, psychology of ethics and theory of ethics of the normal type; not only so, but there are references of contempt for those branches of study. Yet the author has followed recent discussions sufficiently to cast in his lot thus: "The psycho-physiological theory of moral conduct, as also of crime, is dependent on the psycho-physiological theory of 'affect.' The ordinary psychologist devotes altogether too much attention to the examination of the intellectual side of consciousness; the student of criminology cannot thus ignore the passive 'affect'" (p. 252).

In concluding, the author returns to theoretical discussions. He says: All scientific inquiries into man's conduct must necessarily assume the deterministic theory as the correct one. If there can be any scientific account of the criminal's conduct at all, Lombroso's theory should not be objected to on the score of its fatalism (p. 263).

The author describes his work as an outline of methods, results and fundamental principles in criminal anthropology and criminal psychology. He admits that there are two schools, the Italian school, following Lombroso, endeavoring to establish heredity as the full explanation of the genuine criminal, whom they term *delinquente nato;* excluding from the explanation the influence of environment and the results of educative forces; the other, the French school, giving a large place to the influence of environment and the significance of educative forces in their explanation of the adult criminal.

Now, in a general outline of methods and results, we naturally expect to see the results and arguments of both sides fairly and fully presented, and then a decision given in favor of the one regarded as most satisfactory. But Dr. Kurella announces at the outset that he has adopted Lombroso's position, stands forth as an advocate and defender of his assumptions, and then gives tabulated results, mainly selected from Lombroso and those writers who agree with him. This it seems to us, is not giving an outline of criminology, but rather the details of a defense of the theory of one party of criminologists. Almost anything may be proved in this way if one may be allowed to select the facts and neglect everything

that does not fit the theory espoused. It is significant, too, that the cases examined personally by Dr. Kurella were insane criminals. Surely we need a more scientific selection under the term 'criminal.' When we examine the accumulation of evidencc and argument brought forward to establish Lombroso's 'born criminal'—*delinquente nato*—we find that it is all based on the examination of adult criminals who have become habituated to crime. Lombroso and Kurella, viewing these adult (and sometimes insane) criminals with settled tendencies and formed habits, conclude that they are impervious to social reformatory influences; then they conclude that they must have been at birth incapable of education to good habits, fatally predetermined from the first to evil and to evil only. Everything in connection with their character is to be explained from heredity, nothing allowed to environment and educative efforts.

This, it seems to us, is a fallacy in the speculative sort which Dr. Kurella is fond of deriding. How can they rule out the influence of environment and education when, as a matter of fact, these criminals have been in an environmemt and under a training towards vice? If these criminals, tabulated, had been in a contrary environment and under proper social influences from birth, and then in spite of this became irreclaimable criminals, there might be some plausibility in the view that the genuine criminal nature is inborn and is utterly incapable of being essentially modified. When it comes to Lombroso's theory, however, Dr. Kurella seems to be as fatally predetermined to it as the criminal *delinquente nato* is to crime. Bring forward any number of cases of reformation of hardened criminals, he calmly rejoins: we never denied that ordinary criminals might be reclaimed; these were not criminals *delinquente nato*.

In spite of these defects in theory, the work is one that should be widely read, not only by those to whom the author appeals, but also by psychologists and moralists. It will excite many new thoughts, suggest new phases of old problems, and help to indicate how wide a field still awaits the trained psychologist and moralist in the investigation of the psychology and psychology of ethics of the criminal.

JAMES GIBSON HUME.
UNIVERSITY OF TORONTO.

On the New Use of Some Older Sciences. CHARLES L. DANA. Reprinted from the Medical Record, Dec. 15, 1894. New York, Trow Directory Co. Pp. 19.

Dr. Dana's article is a summary of the results obtained by Lom-

broso, Féré, and others, from anthropometric examinations of criminals. In addition to the well-known facts about the shape of the skull and the facial expression, he discusses a number of minor marks: the shape of jaw and cheekbone, the ridge along the centre of the palate, the shape of the ear, length of third finger, prehensile foot, etc. The author differs from the Italian school in classing together criminal, insane and neuropathic degeneracy and refusing to consider these as different types. From an ethical standpoint the presence of these marks of degeneracy in an individual "throws an additional responsibility upon him. * * * We do not excuse the cripple who attempts to become a sprinter, nor should we excuse the morally defective who * * * fail to husband the endowments they possess." A few such sentiments as these will go a long way toward justifying this science in the eyes of those who are accustomed to regard it as an example of *fin-de-siècle* morbidness.

H. C. WARREN.

NEUROLOGY AND BIOLOGY.

Il Cervello in relazione con i fenomeni psichici. G. MINGAZZINI. Turin, Fratelli Bocca, 1895. Pp. 204.

This interesting study on the morphology of the cerebral hemispheres of man forms the twenty-second volume in a 'Bibliotheca Antropologica-Giuridica,' a series to which the best known of the Italian workers in this field have already contributed. The problem attacked is the one which dates at least from the days of Erisistratus—namely, the interpretation of the gyri of the cerebral hemispheres.

To approach this question properly the comparative anatomy and embryology of the primate brain is needed; and assuming both interest and knowledge, the author attacks his subject without further preface, comparing the fœtal human, with the fœtal primate brain, so far as material will permit. The relation found in the length, angle, position and connections of the important sulci is such that, in those primates more removed from man, there is far more divergence from the human type than exists between man and the highest primates. In ontogeny the similarity in development becomes less with the increasing age of the individuals, and soon any form begins to exhibit those features which are distinctive of it at maturity. Thus the characters of the cerebral surface in man are not superadded to those found in the higher primates when adult, but to cerebral features exhibited by them when still in the fœtal stage.

This is a relation which is perfectly familiar and well recognized in the case of other organs, but it has never before been thus demonstrated for the cerebrum.

With the data gathered in this chapter, including many tables of measurement new and old, the author next attacks the peculiarities of the cerebral hemispheres according to sex. The use of fœtal material brings out the fact that, in the case of the female, the seventh to eighth month of fœtal life represents a period of accelerated growth for the cerebral fissures. Some fissural variations occur which are characteristic for sex. The definite marks are slight, however; and even in the case of the historic question of the relative development of the frontal lobes, the evidence is still inconclusive. As regards race differences, there is little to be said, since the only way in which peculiarities of fissuration according to race can be given a value is by estimating the relative frequency of their occurrence; and at present the material available from non-European races is too small to warrant a statistical use of it. M. suggests that the brain in the inferior races is wont to show with unusual frequency variations indicating arrested development; but even if unduly frequent, such arrest is certainly slight. When the brains of eminent men are compared with those of persons with average intelligence, the former are found to be more amply developed. The third frontal gyrus contributes to this difference, and the variations are in this locality particularly noticeable. It is to be remarked, however, that just this gyrus is one of the last to be completed, and any differences in growth would therefore be most easy to detect; whereas changes of the same kind occurring at other localities might be more readily overlooked. In cases where the head has been deformed, there are but few records concerning the brain surface. The studies of Ambialet on the '*Déformation toulousaine*' being by far the most important. In such instances the effect of deformation is both general, causing an incompleteness in the later formation of the gyri and local, as indicated by the fact that, along the line of the constricting band, the arrest is most marked. Most instructive is the author's chapter on the criminal brain, since the data for comparison, in the form of well-constructed tables of measurements, are very full. When thus examined the criminal brain exhibits no features which are typical. Among the brains of criminals are to be found some which show an arrest of fissural development, but more than this cannot be said. It may be added that the theory of confluent sulci and theromorphic fissuration are again left without support; and the contention of Benedikt, that such were

the characters of the criminal brain, is as completely refuted by this last study as it already has been by the previous studies of Giacomini, Eberstaller and Cunningham.

In reviewing the condition of the hemispheres in the insane, the deaf-mutes and the microcephalics, the author brings together, especially under the last head, a large number of cases. In all these groups errors of development are the only recognizable features, and these affect principally the later stages of growth. The microcephalics exhibit the most diverse forms of fissuration, the value of which may, so far as the brain is concerned, be estimated on the basis of the comparative embryology of the primate brain, but the phylogenetic value, in a broad sense, will depend on the anomalies of the other systems correlated with them. Pithecoid characters having an atavistic value are rare in idiots, rarer in the insane, and in criminals have never been described; whereas those variations which indicate an arrest of development are frequent among the insane, more frequent among idiots, and most frequent among the microcephalics. As to the cause of microcephalism and the brain-markings characteristic of it, these are to be referred to some pathological condition which prevented provision for the later periods of growth, during the early history of individual development.

H. H. D.

Les localisations cérébrales en psychologie. G. HIRTH. Paris, Alcan, 1895.

The author informs us that his aim in this book is to show the possibility of applying the theory of localization to the problems of psychology. We do not believe that he has succeeded in his end; for, in spite of certain ingenious ideas which one comes across here and there in the reading, one closes the book with no clear idea of his theory. As regards the assistance which psychology is to get from physiology, there is no need of exaggerating it. It was the fashion some years ago, in the midst of the reaction against metaphysics, to treat psychology along with physiology; but we are aware to-day that that contest is over, and that experimental psychology has acquired its independence. We are able, therefore, to add that the theories of cerebral localization, which are still hypothetical, do not interest psychology in any direct or fundamental way. Properly controlled, introspective researches afford us much more profitable avenues into the heart of our study. A. B.

Hibernation and Allied States in Animals. WESLEY MILLS. Trans. Roy. Soc. Canada; Section IV, 1892. Pp. 49–66.

Dr. Mills reviews the evidence regarding the nature and duration of hibernation in the woodchuck, squirrel, black bear, bat, and various kinds of fish. Passing to the discussion of similar states in man, he instances three cases, one of which came under his personal observation, of individuals who remained for long periods in the trance state. The case referred to is described at some length; the patient, an old woman, had remained in a comatose condition for thirteen years; the autopsy showed the absence of any gross lesion of the brain.—Any explanation of the nature of hibernation must take account of the fact that all degrees of functional cessation are found, from normal sleep to the lowest degree of activity consistent with the maintenance of life. There is abundant reason to connect these states together. In sleep the functional activities are lowered; the animal, therefore, requires much less food, and its excretions are greatly diminished. The same laws apply to trance and hibernation. The hibernation of amphibia, reptilia, etc., is protective, being the result of adaptation to environment; this explains equally well why the trait may be lost when the animal is transferred to a different environment. H. C. WARREN.

VISION.

Über die latente Hypermetropie. CL. DU BOIS-REYMOND. Zeitschr. f. Psych., VIII, 34–43. 1894.

When a hypermetropic person makes use of glasses for the first time, he is at first unable to accommodate sharply for the furthest portion of his new accommodation-field; but after short use he gains control of a portion of this, differing in extent with his age. The author attempts to explain this phenomenon. By extending Donder's presbyopic curve into the first years of childhood, he finds a value of about twenty diopters for the earliest accommodation. Dividing the total activity of the ciliary muscle into twenty parts, he calls each of these units an entony (Entonie), and assumes that these units are approximately equal. He further assumes that presbyopia is dependent only on diminished elasticity of the lens, and not at all on a weakening of the ciliary muscle. In case a child is hypermetropic, it fails to gain practice in making use of the first entonies, in number corresponding to the diopters, which are unused; the whole hypermetropia is latent, and a practical far-point of accommodation is formed. If in later life glasses are used, these

first entonies cannot be accurately controlled; but the number of diopters to which they correspond (the latent hypermetropia) diminishes with age, since with diminishing elasticity of the lens the available diopters decrease while the entonies remain constant. We may then formulate the following theory: The practical far-point of accommodation, corresponding to the latent hypermetropia of the first year of life, remains constant for the muscular activity (*i. e.*, measured in entonies) during the whole life, and forms the limit between the manifest and the latent hypermetropia, provided no glasses have been used and the refraction has not changed. The latent hypermetropia, diminishing with age, bears a proportion to the total hypermetropia expressed by the ratio A : 20, where A is the accommodation, measured in diopters, belonging to each age.

The author shows that, although the theory is hypothetical, it yet explains all the facts and accords with the actual measurements of latent hypermetropia which have been made by Hirschberg and Daniel.

On the Relation of Accommodation and Convergence to our Sense of Depth. E. T. Dixon. Mind, N. S., IV, 195–212. 1895.

Dixon repeats and extends the experiments, and criticises the conclusions, announced by Hillebrand in an article of which an abstract is given in this Review, Vol. I, p. 540. H.'s experiments led him to the belief that when all aids except convergence and accommodation are excluded, we cannot judge distances monocularly; and that impressions of distance are therefore not produced by movements either of accommodation or of convergence. Dixon used the same methods as did H. for excluding 'empirical' factors, with some improvements. His results are also largely the same. When the object of fixation was moved to or from the observer while he was watching it, there was no accuracy in judging the direction of movement (first series of experiments). When the object was rapidly moved out of the field of view, and another at a different distance was substituted for it (second series), Dixon found: (1) that each of his three observers was able in some degree to judge distances monocularly, but the power of doing so varied greatly in the three cases; (2) that the judgment was directly or indirectly based on the different accommodation required for different distances; (3) that the actual criterion seemed in all three cases a difference in the rapidity or ease with which the accommodation adjusted itself or was adjusted by the observer, and not (as H. explains these results) in any conscious direction of the accommodation by the

observer; (4) all three observers commenced by interpreting the criterion in the right way (showing its association with the impression of depth, at least unconsciously, in ordinary life), though later they differed; (5) Wundt's observation that changes from far to near are more easily observed than changes from near to far, is not confirmed in every case; (6) an absence or defect of accommodating power may enable a person to judge distance monocularly, but this is not the way people with normal eyes judge.

Hillebrand's conclusion is based entirely on the negative results of his first series of experiments. But Dixon shows that the association between convergence and accommodation is a loose one,—in his own case he found that even at 100 cm. distance he could make a difference of over six diopters in convergence without affecting accommodation; that if, in judging distances monocularly, convergence does not actually follow accommodation, it might be that wrong answers were given in just those cases where the convergence went wrong; he found, however, no method of observing with certainty how far this was true; and that the results might also obviously be attributed, not to absence of, but to too gradual change in, the feelings of accommodation and convergence. As to the second set of experiments, the fact that H. "was able to give another explanation, however plausible, of the successes, cannot be held to prove that sensations of convergence are non-existent;" and the facts under (3) above do not uphold H.'s explanation. These experiments do not, therefore, prove, as H. thought they did, that muscular sensations are of no aid in our judgments of depth.

Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten, und ihrer Combinationen. J. COHN. Philos. Stud., X, 562–603. 1894.

Cohn investigated on fourteen persons the relative agreeableness of different colors and of different combinations of color. His results were: (1) Of two shades of the same color, the more saturated is the more pleasing. Among different colors of approximately the same saturation, the preference appears to depend on individual tendencies, with the exception that yellow is for most observers less pleasing than the others. (2) In case of equally pleasing components, a combination of two colors is the more pleasing the greater difference there is between the components. (3) Two colorless brightnesses are the more pleasing in combination the greater their difference. In comparing single brightnesses, white pleases more than gray or black. (4) If we combine a color with a varying color-

less brightness, or two colors of which one varies in brightness, there is a preference, though less clearly marked, for the greater difference in brightness.

Even in cultivated people, therefore, accustomed to quiet colors and to slight color differences, there exists a preference for saturated colors and for strongly-contrasting combinations, when there is question only of the purely sensory effect, without other considerations. A great many secondary influences, variable and difficult to determine in full, affect the judgment, but their disturbing effect can be overcome or diminished by a sufficiently large number of experiments. These preferences are to be explained, not by any analogy with musical harmony, nor by the effect of simultaneous contrast, but by the fact that differences of sensation as such produce pleasurable effects, entirely apart from the modification of the sensations by one another.

E. B. DELABARRE.

BROWN UNIVERSITY.

CONSCIOUSNESS AND IMAGINATION.

Imagination in Dreams and their Study. F. GREENWOOD. London, John Lane; New York, Macmillan & Co., 1894. Pp. ix + 198. Price, $1.75.

The author's modest purpose 'being only to convey gossiping hints and suggestions to the generality,' a minute examination of his book would be ungracious; but it must be said that, as a book for the generality, it has serious shortcomings. With the main thesis that dreams deserve a more thorough and rational study than they have yet for the most part had, no fault can be found; the author's criticisms are often just, and some of his suggestions illuminating; but his psychology is antiquated—often more like that of Bunyan's Mansoul than that of the present; and too prominent a place is given to dreams 'which have all the character of prophecy and revelation.' It is to be regretted that one so well equipped in other directions, as Mr. Greenwood seems to be, should not have had the psychological training that would have enabled him to make a really valuable contribution to the scientific study of dreams, which he so ably advocates.

EDMUND C. SANFORD.

CLARK UNIVERSITY.

Mémoire et Imagination (peintres, musiciens, poètes et orateurs). LUCIEN ARRÉAT. Paris, Alcan, 1894. Pp. VII + 171.

As the author himself tells us, this is only a preparatory study,

an introduction to the more precise investigation, by means of laboratory and personal methods, which the topic demands. But even with such modest pretentions the book does good service, for it presents many subtle and ingenious ideas. The first part, devoted to memory, includes musicians, poets and orators. M. Arréat examines them in turn on motor, visual, auditory, emotional, and intellectual memory.

It is impossible to sum up briefly so many facts, but we may note in passing that Victor Hugo had enormous command of verbs, but no exceptional stock of qualifying adjectives of a visual kind.

The second part, devoted to imagination, appears to be quite as interesting as the first, and newer. He includes under imagination the facts of manual skill, the movement-memories of the fingers of the designer, the rich verbal flow of the orator, the incidents of his piece in the case of the dramatist. Then he describes the creation of a work of art (p. 127): 1. The conception, whether conscious or not, finished or not; 2. The execution, which is not alone the development of the thought: it reacts upon the thought and modifies it; 3. The emotional state which accompanies the execution; 4. The critical judgment by which the author accepts or rejects what comes to him; 5. A certain doubling of consciousness which results in two different attitudes of mind. Finally, the author draws an interesting analogy between imagination and delirium, and shows that the difference resides in the evident control by the intelligence in the former. A. BINET.

In what Sense are Psychical States Extended? F. H. BRADLEY. Mind, N. S., IV, 225–235. April, 1895.

In a previous article Mr. Bradley discussed the intensity of mental states, maintaining that they possessed degree in various respects, and were in principle, if not in actual practice, measurable quantities. Incidentally he asserted that they, or at least some of them, possessed extensive quantity as well (see this REVIEW, II, 319). To this last point he now recurs. The general principle is that ultimately everything is psychical; but for the purposes of the discussion the less sweeping form of statement is adopted—that any feature of content which makes the meaning of an idea must be present psychically. It is on this principle that mental states are said to have weight and odor, to be long and broad. In what sense, then, is the extension which is predicated of a physical object also in the soul? It is true the physical extended is not present psychically in its full process, and there may even be doubts as to the

extent to which we possess the abstract feature of its extension; still, up to a certain point at least, the extension which we have psychically present is the same as that which qualifies nature. The objection that if psychical states are extended they will collide with one another or with other extended things, is groundless. Spatial things need not be related spatially at all. The soul contains many disparate extensions. Is, then, the soul extended? In respect of certain of its states, the answer must be as given; but in itself, and as a whole, the answer is emphatically, No. For extension is for it not an all-pervasive, but only a particular and subordinate quality; whereas in the physical world it is taken as primary and predominant.

By way of appendix, Mr. Bradley formulates his doubts respecting the 'extensity' regarded by James and Ward as lying at the basis of our perceptions of space. These writers, he says, unite in what are perhaps two errors: claiming to observe extensity as a fact and denying in effect all non-extensive volume. Volume, he finds, indeed, everywhere, but either as implying space outright or as involving something less than extensity. By extensity he apparently understands the quality of 'side-by-sideness.' Nevertheless, while declining to identify volume with extensity, he goes on to mention two aspects of volume—viz., 'its intensiveness and its extent' as present and given, but not distinguished and developed, even in *mere* volume. But is not this undistinguished and undeveloped aspect of extent present, as Mr. Bradley implies, in all sensations, precisely what Ward, wrongly, perhaps, denying plurality to the intensive aspect, means by that 'latent or merged plurality' by which he defines 'extensity?' Whether extensity, in this sense, can be now observed is a separate question. Mr. Bradley admits that some such quality must be postulated.

H. N. GARDINER.
SMITH COLLEGE.

EXPERIMENTAL.

Ueber die Beeinflussung einfacher psychischer Vorgänge durch körperliche und geistige Arbeit. SIEGFRIED BETTMANN. Psychol. Arbeiten, I, 152–208. 1895.

Herr Bettmann's paper is a study of the mental fatigue effect of physical and psychical work. To secure uniform conditions, the experiments were all made immediately after awaking, the duration of sleep being carefully regulated. The subject (the author himself) arranged his mode of life methodically, avoiding any excite-

ment or labor outside of that required for the tests, and abstaining from tea, coffee and stimulants during the entire period covered by the experiments. The mental labor consisted in adding columns of figures for an hour; the physical labor chosen was to walk rapidly for two hours. The two tasks were performed on different days; at the close of the exercises a series of Choice Reactions were made. To furnish material for comparison, a reaction series was made on other days at the same hour without preliminary exercise. The subject had already reduced his reaction-time by weeks of preliminary practice. The results show: (1) for normal days (*i. e.*, without exercise) a slight reduction of the average time during the first sixty reactions, then a gradual increase to about 300 σ at the end of 300 reactions; (2) for the days of mental exercise, a very constant average of about 380 σ during the entire series of 300 reactions; and (3) for days of bodily exercise, a steady *decrease* of the times from about 300 σ to 230 σ—some of the figures running as low as 190 σ. On the other hand, the last case shows about 25 per cent. of anticipations to 1 per cent. for the tests following mental exercise, and 2.5 per cent. for normal days.

The results indicate that mental impairment follows from physical as well as from psychical labor; in the latter case the reaction-times are shorter, but the enormous number of anticipations neutralize any gain from this source. Some further experiments, in which the tests were word-reactions and the facility of learning twelve-place numbers respectively, serve to accentuate this effect: as the difficulty of the tests was increased, the apparent advantage of the physical exercise was rapidly lost.

The author criticises the results of Mosso and Vintschgau, on the ground that their conditions of experimentation were too complex to admit of discrimination between various affecting influences; in his own experiments the conditions were as simple and uniform as it seems possible to make them. H. C. WARREN.

Minor Studies from the Psychological Laboratory of Clark University: under the direction of E. C. SANDFORD. VIII. *A Study of Individual Psychology*, CAROLINE MILES. IX. *The Memory After-Image and Attention*, A. H. DANIELS. X. *On the Least Observable Interval between Stimuli Addressed to Disparate Senses, and to Different Organs of the same Sense.* XI. *Notes on New Apparatus*, E. C. SANDFORD. Am. Jour. Psych., VI, 534–584. Jan., 1895.

VIII. The psychologist cannot hope to solve all his problems by

experiments in the laboratory. Some questions can be solved only by introspection. If, however, introspective results are to be of value, they must be confirmed by others. The method advocated by Miss Miles is to make a list of such introspective questions, and obtaining answers from a large number of people, to secure thus a vast amount of valuable data. Of course there are difficulties. The answers of untrained observers are naive and unreliable. Many subjects are unconsciously influenced either by aversion to revealing an undesirable phase of their inner life or by the desire to give results satisfactory to the experimenter. Notwithstanding these difficulties the method is valuable; more, however, for its suggestiveness than for the actual results obtained.

The following headings will indicate the character of the questions asked of 100 Wellesley women: I. Habits of discrimination and memory; II. Methods of concentrating attention and of getting asleep; III. Emotions and preferences; IV. Recollections of childhood; V. Miscellaneous.

IX. These experiments (as those of XI) are interesting from their bearing upon the attention. This study aims to estimate the duration of memory after-images—a phenomenon familiar to all psychologists. The memory after-image is differentiated from memory by association—by distracting the attention as far as possible from the sensation when received. A group of three digits was pronounced while the attention was distracted by the reading aloud of interesting stories, and the subject was required after a given interval to reproduce them. The intervals were 0, 5, 10, 15 and 20 sec.

It was found that the longer the interval the less probability there is that the reproduction will be correct. Less than 15 sec. is the limit; beyond this the images could not be reproduced. If perfect distraction could be attained, this limit could be decreased almost indefinitely. If, however, the digits return to consciousness before the given interval has elapsed—to which, as it is important to note, there is a great tendency—the limit is considerably extended (perhaps to 20 sec.) and greater exactness is attained. As we should expect, there are more errors with the first and second digits than with the third. The article concludes with a comparison of these experiments with those of Dietze and Wolfe.

X. "The object of the following experiments was a remeasurement of this interval for single pairs of stimuli under varying conditions of the attention." In the first series, with unforced attention, an important result is the unanimity of the requirement of a longer interval for the 'click-flash' combination than for the 'flash-click.'

This is interesting, as it is 'flatly opposed' to the results of Exner in all his seven subjects. Exner's data being false, his explanation that the F.-C. requires a longer interval, because of 'the slower rise and greater duration of the visual sensation,' is, of course, invalid. The author favors the explanation of the differences in the intervals by the movements of the attention (Wundt) rather than by 'sensory inertia.'

In the second series, with forced attention, it was found, contrary to expectation, that voluntarily attending to a specific stimulus has no appreciable tendency to make it appear to precede the other. The best results were obtained, not when the attention was preadjusted, but when the subject was in a state of 'alert indifference.' An explanation of this preadjusted attention is suggested. We decide upon the order of the occurrence of stimuli, not by the order of the sensations resulting thereby, but by the order of the sensations from the reflex movements in the adjustment of the organs in response to the stimuli. If the attention be preadjusted, the sense-organs are more or less adapted, and consequently the reflex movements being more obscure, the judgment is correspondingly uncertain.

XI. Four new pieces of apparatus are here described. The Binocular Stroboscope is an instrument devised by Prof. Sandford for demonstrating a 'little-known' phenomenon of binocular vision —viz., the definite localization of a moving object due to an instantaneous glimpse by each eye in succession. There are also described, 'A Model of the Field of Regard;' 'A Simple Adjustable Stand,' which can be put to a variety of uses; and 'The Pendulum Circuit Breaker,' an instrument "designed to break electrical currents at known and regulable intervals of time from one another." This last instrument was used in the experiments of Study X to obtain an exact interval between the application of the different stimuli. W. J. SHAW.

Minor Studies from the Psychological Laboratory of Cornell University. Communicated by E. B. TITCHENER. VI. *Taste Dreams.* E. B. TITCHENER. VII. *On the Quantitative Determination of an Optical Illusion.* R. WATANABE. VIII. *The Cutaneous Estimation of Open and Filled Space.* C. S. PARRISH. Amer. Jour. of Psy., VI, 505–523. Jan., 1895.

VI. This study aims to show that the explanation of dreams, adduced in connection with the other sensations, is adequate for gustatory dreams also. Dreams are of two kinds: presentative

(occasioned by a peripheral stimulus) and representative (due to central excitation). The tendency of psychology is to emphasize more and more the relative importance of the peripheral stimulus; thus the *Eigenlicht* of the retina explains the great predominance of visual dreams.

The number of taste dreams is relatively small. Miss Calkins (A. J. of Ps., V) reports only two in a total of 335 dreams, and none in a total of 298. While without doubt the majority of dreams of the gustatory type are representative, we have no right to assume that they are all presentative any more than we have to assume that all visual dreams are presentative, because the retina is in continuous excitation. In conclusive support of this, five taste dreams were collected, all representative. Two of these are described—one a case of auto-suggestion, being experienced by the author himself.

VII. This article is a continuation of an investigation of the illusion of point and dotted line distance begun by Mr. Knox (A. J. of P., VI, 3). The apparatus consisted of a set of cards, on each of which was placed a line, made up of a dotted line, and a point distance, the centre of the line coinciding with the centre of the card. Knox, by keeping the point-distance constant, and varying the dotted line until the subject judged them equal, found, besides confirming the illusion itself, that the mean variation of the vertical differences is not greater than that of the horizontal. This result was unexpected, because "binocular bisection of horizontal distances is not subject to any constant error," while of vertical lines we constantly over-estimate the upper part.

Dr. Watanabe, 'in the hope of elucidating this difficulty,' conducted a similar series, except that for him the dotted line is the constant. His results on three subjects confirm those of Knox, in showing not only that the illusion itself prevails in all cases, but also that the vertical differences have not a greater mean variation than those of the horizontal. In regard to this latter, he says: "The fact itself holds, [but] we are not at present prepared to suggest any explanation."

VIII. It has been amply proven by experiment that, to the eye, filled space invariably appears greater than empty space, objectively equal. This has been explained by the fact that the movements of the eye are more retarded in the case of filled space. Dresslar (A. J. of Ps.) maintains that the same illusion holds in the case of touch, and the same explanation—viz., movement plus touch—is adequate. Prof. Parrish maintains that, so far from Dresslar's explanation

being adequate, the alleged facts on which it is based are not true—*i. e.*, filled space is not judged by touch as greater than empty space. On the contrary, he shows by two series of experiments upon seven reagents that filled space is judged actually shorter.

In the first series, nine pieces of wood were used, to each of which was attached a number (varying from two to nine) of small rubber projections, the distance between the extremes being constant—64 mm. These were applied to wrist and forearm. The result shows "that for a resting skin a filled distance is, on the average, shorter than an empty distance objectively equal to it."

In the second series, the apparatus consisted of a strip of rubber .01 in. in thickness (for filled space) and an æsthesiometer (for empty space). The results here confirm the first; filled space is invariably judged shorter than open space—*e. g.*, "an open space of 24 mm. is judged equal to a filled line of 28 mm., and one of 15 to a filled line of 20." The author concludes that these experiments upon resting skin, showing an illusion opposite to that of sight, clearly support the explanation of the latter in terms of movement.

WESLEYAN UNIVERSITY. W. J. SHAW.

Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Schalleindrücken. Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Licht und Tasteindrücken.—J. J. VAN BIERVLIET, (Phil. Stud., X, pp. 160 ff; XI, 1, pp. 125–135.

In these two papers are communicated the results of the first part of an investigation undertaken by Professor van Biervliet in the psychological laboratory at Ghent to determine the influence of the circulation of the blood upon the length of the reaction time. The present communications report upon modifications in the duration of the simple reaction to stimuli of sound, light and touch, accompanying varying rates of pulse beat. Further investigation will concern itself with the effects of changes in blood pressure.

In all, 17 different observers took part and the results of 6800 experiments are reported. The subjects were given preliminary practice. The experiments were made in series of 20; before each series, the pulse rate at the wrist was determined by two independent observations. The ordinary set of apparatus was employed, including the Hipp chronoscope, a hammer to give the sound stimulus, the Leipzig pendulum for the light stimulus, and for touch a hammer, which was allowed to drop upon the first joint of the middle finger. The subject sat in a dark room apart. In the

sound series, each subject made 400 reactions in all; in the light and touch series, results are collated from 200 '*useable*' reactions of each individual. The reactions are designated as *sensory*. The bell signal was given 5 seconds before the stimulus in each experiment. In the sound series the stimuli followed one another at one minute intervals; in light and touch, at 15 seconds.

Typical results are the following: In the sound series the reaction time of one subject diminished from 130 to 117 as the pulse rate increased from 70 to 110 beats per minute; a second subject decreased in reaction-time from 144 to 130 with a pulse rate increasing from 70 to 100; a third from 154 to 130 with pulse rate increasing from 70 to 100. With the light stimulus, a pulse rate increasing from 60 to 80 gave a reaction-time decreasing from 187 to 174; one of 60 to 110 gave reaction-time of 208 to 123; a third increasing from 80 to 100 gave decreasing reactions of 209 to 176. With the touch stimulus, pulse rate increase from 60 to 100, reaction-time decrease from 209 to 176; pulse rate increase 70 to 90, reaction-time decrease 186 to 150; pulse rate increase 80 to 100, reaction-time decrease 167 to 164; pulse rate 80 to 100, reaction-time 175 to 164.

From these results, Professor van Biervliet concludes that as a rule, the time of a reaction diminishes with an increase in the pulse rate. He finds however that with the pulse at a minimum and especially at a maximum, the variations in the reaction-time do not follow this rule. Differences in pulse rate, therefore, may produce remarkable changes in the duration of the reaction-time. In all investigations into the phenomena of attention in which the length of the reaction-time is used as a means of analysis, it would seem necessary in consequence to give some consideration to the condition of the pulse.

These conclusions of Professor Biervliet are not justified by his tabulated results. The average results of the individual series do follow his theory closely, but the large mean variations in conjunction with the small differences in the averages of reaction-times, make the experiments entirely negative. In nearly every case the mean variation of the single experiments from the average results of the series is larger than the maximum difference in reaction-time for the extremes of pulse rate. A pulse of about 70 beats per second gave an average reaction-time of 130, while one of 110 beats gave 117. This result may or may not be significant of real difference in reaction-time, depending upon the number of experiments composing the average result and the size of the mean variation of the individual experiments from this result. In the case taken, the

mean variation was about 30. This result only signifies that with a pulse of 70 beats, the reaction-time lies between 100 and 160, while with one of 110 beats it lies between 87 and 147. There is no experimental evidence in the result to show that with a pulse beat of 70 the reaction-time may not be 100 and with one of 110 it may not be 147. The difference between a reaction-time of 167 and 164, both averages obtained with mean variations of 16, and that between 175 and 164 averages with mean variations of 18, are purely accidental. To prove that results differing so slightly are significant of a real difference, the mean variation must not be greater than 1 or 2 and the experiments must be more than a few score in number.

In the interests of psychometry, it may be well to call attention to the importance of giving the mean variation of the recording instruments. It is very probable, that a large part of the variation in the results is due to the variable error of the apparatus. This ought to be greatly reduced. The number of practice experiments ought to be given, and the results of practice series ought not to be rejected from the final showing. It is desirable to know what is the nature of the practice in which the *subject learns to react* and what is the effect of that practice upon the result. Further, the value of all results would be increased if it were reported how many and under what conditions results were excluded "*because they varied too much from the average*".

UNIVERSITY OF PENNSYLVANIA. LIGHTNER WITMER.

MOVEMENT.

L'Écriture et le caractère. J. CRÉPIEUX–JAMIN. Paris, Alcan, 1895. Pp. 442.

Although there is no such science as graphology, and no such men as graphologists, and although it might well be asked what interest attaches to the study of handwriting, nevertheless it is certain that the study itself is attractive and useful. Handwriting involves the most complicated and delicate of acquired movements: it is comparable in this respect with the movements necessary to hold and play an instrument; besides, as its purpose is the translation of thought and emotion, it possesses an expressive function, just as mimicry and gesture do. These two reasons appear to me to be more than sufficient to make desirable the profound study of handwriting. But a second question arises; that of method. It is evident that a simple method is necessary here as elsewhere; that of observation, first of all, of writing as a kind of movement, with an

analysis of the different elements of movement which enter into it; taking account of the pressure, rapidity, etc., with description of the form. There are psycho-physical instruments which might be used for this analysis. Then, following this study of normal writing, that of its various modifications, especially the influence of age. What are the principal marks of handwriting from the first pothooks of the child? What are the modifications due to old age? Are there family characteristics? Can we detect any hereditary influence apart from the influence of imitation? Then would come the disturbances of handwriting: the influence of cold, fatigue, alcohol, and various poisons, disturbances due to disease, etc. In short, handwriting should be put to the same series of tests that all physiological facts are put to.

It must be confessed that psychologists have rarely followed such a truly scientific method. And although M. Crépieux-Jamin is undoubtedly one of the most serious and exact of graphologists, he yet makes no use of the method of observation and experiment, except incidentally. Indeed, research in this subject seems to have been badly oriented from the first. The most complex and difficult questions have been put, such as the reading of the writer's character in his handwriting. We have all had curiosity in regard to letters, and have tried to attach to the writing some information about the writer; but to understand the difficulty of it, one has only to call to mind what a complex thing we mean by character. There is nothing more complex, more changing, more vague. So how can we attack such a problem? The early graphologists did not state their method, if, indeed, they had any. They took the attitude of those who found it necessary to keep a monopoly. Their successors, especially Lavater and the Abbé Michon, did make their method known; and it may be described in two words—intuitive and analytic. The first describes a sort of subconscious divination founded on long experience. The second process discovered a certain number of signs in the writing, each of which is capable of direct interpretation. M. Crépieux-Jamin adopts this second method, although making a certain use also of intuition. He reaches the most minute rules of procedure. In order to reach good results, he says it is necessary to have a large number of writer's specimens; while the writing on official documents is worthless, that of private letters is most valuable. There are three stages in the interpretation of the signs: first, general signs, such as clearness, angle of the letters, etc. Then come particular signs, such as the identity of 'n' and 'u,' that of the way 't' is crossed, etc. And, finally, in

order to complete the psychological portrait of the writer, one must study the evolution involved—a very delicate task—which consists of a judgment of the effect produced upon the writing by the addition of one mental quality to another. . . . Some of the author's analyses under this last head are very fine, and this part of his book is very pleasant reading. But from the point of view of science, the newest portion of his book is useless—that which deals with handwriting under irregular conditions. In it he finds thirty-nine different signs, each of which has a special psychological significance.

All this does not constitute science; it is simply exposition. And the tone of authority which may be put into an affirmation does not increase its demonstrative force. The feeblest mar of the bookk is lack of proof. . . If we should attempt to show what the author has not done in this respect, we should begin by citing the proposition that writing is a kind of gesture or a series of small gestures; and that the relation of writing to character can be made out only on the basis of this analogy with gesture (p. 37); so far we go with him. But he then establishes a series of presumptions on the probable value of writing. . . And in it all, as in his treatment of graphic signs, he gives only what is vague and ambiguous.

Yet he has collected in his useless chapter on 'Experimental Graphology' some interesting experiments made upon himself. He tried writing five or six lines under varying conditions, such as heat, cold, fatigue, joy, and sorrow, etc. The idea is excellent, but it is to be regretted that the experiments were not repeated on persons who did not have preconceived ideas; and that these experiments, which are difficult to get, were not made the real basis of his book. Such experiments, of course, would not give the key to character; to study that question, hypnotism might be called in as a method. We might give to proper hypnotic subjects suggestions of simple emotions; see that they realize them; and then set them to writing under the influence of these emotions. Then we might compare different specimens of such writing. As I said ten years ago, that would be the best method of experimental graphology.

SORBONNE, PARIS. A. B.

ETHICAL

The Study of Ethics. A Syllabus. J. DEWEY. Ann Arbor, Mich.: Register Publishing Co. The Inland Press, 1894. Pp. 151.

This study has as its aim "to justify the belief that amid the prevalence of pathological and moralistic ethics there is room for a theory which conceives of conduct as the normal and free living of

life as it is." It attempts a psychological analysis of 'active experience' and a revelation through this analysis of the 'chief ethical types'. Those who have found the author's former work 'Outlines of Ethics' stimulating, suggestive and original will find here the same characteristics, combined with a genuine philosophic candor and downright manliness of expression. In form and method it is a model of what a syllabus should be. It is a pity, however, that anything so good should be marred by such an absurdly large number of typographical errors.

The contents are divided into nine chapters treating successively of (1) the nature of ethical theory, (2) the factors of moral conduct (3) a general analysis, (4) the moral consciousness, (5) moral approbation, (6) reflective approbation, conscience, (7) nature of obligation, (8) freedom and responsibility, (9) virtue and the virtues. In these chapters we are introduced again to some doctrines already familiar to readers of Professor Dewey's articles in the philosophical reviews, *e. g.*, to the doctrine of the mediation of impulse (will)—"the expression of every impulse stimulates other experiences and these react into the original impulse and modify it. The reaction of the induced experiences into the inducing impulse is the psychological basis of moral conduct. . . . . This back reference of an experience to the impulse which induces it we may term the mediation of impulse"; to the doctrine of the agent's moral situation as "nothing but the complete co-ordination of all his powers (abilities) and relations. . . . The good man in a word is his whole self in each of his acts, the bad man is a partial (and hence a different) self in his conduct."

The critical examination of the hedonistic and Kantian ethics is searching and convincing, both theories being condemned for their abstractness. Green's theory of the moral ideal is also made the subject of an illuminating criticism.

Every student of ethics should read this book. To the reviewer's mind it is one of the most suggestive published in recent years.

MIAMI UNIVERSITY. R. B. JOHNSON.

NEW BOOKS.

Saggi di Psicologia del Bambino. P. LOMBROSO. Turin and Rome, L. Roux, 1895. Pp. XII + 284.

Introduction to Social Philosophy. J. S. MACKENSIE. 2nd ed. London and New York, Macmillans, 1895. Pp. XIV + 454.

The Pathology of Mind. H. MAUDSLEY. London and New York, Macmillans, 1895. Pp. XI + 571.

Philosophie und Erkenntnistheorie. Ite Abth. L. BUSSE. Leipzig, Hirzel, 1895. Pp. VI + 289.

Éléments de psychologie humaine. J. VAN BIERVLIET. Paris, Alcan, 1895. Fr. 8.

Ueber das Grundprincip der Association. ALLIN. Berlin, Mayer and Müller, 1895.

Gewöhnung und Gewohnheit. (Dissertation, Jena.) A. P. KURTIDIS. Athens, Konstantinidis, 1895. Pp. 64.

Die Aufgaben und Methoden der Psychologie in der Gegenwart. J. MERKEL. Zittau, Böhme, 1895. 4to, pp. 36.

Vorlesungen über soziale Ethik. G. v. GIZYCKI. Berlin, Dümler, 1895. Pp. 88.

Zur Psychologie des Schreibens. W. PREYER. Hamburg u. Leipzig, Voss, 1895. Pp. 230.

Die Grundprobleme der Philosophie. I. SOCOLIU. Bern, Beck-Keller, 1895. Pp. XVI + VI + 260.

Princeton Contributions to Psychology. Vol. I, No. 1. Reprinted from PSYCH. REV. Edited by J. MARK BALDWIN. Princeton University Press, 1895. Pp. 44.

Die Zweckmässigkeit der psychische Vorgänge als Wirkung der Vorstellungshemmung. O. LINDENBERG. Berlin, Duncker, 1894. Pp. 64.

The Essentials of Logic. B. BOSANQUET. New York, Macmillan, 1895. Pp. X + 168. $1.

Transactions of the American Medico-Psycholog. Association. Vol. I. 1894 (Semi-Centenn. Proceedings). H. M. HURD, Sec. The Assoc., Utica, N. Y., 1895. Pp. X + 361.

NOTES.

Prof. W. L. Bryan is now Vice-President of the University of Indiana and Professor of Philosophy; Dr. John A. Bergström is Assistant Professor of Psychology and Pedagogy, and Mr. E. H. Lindley, Instructor in Philosophy, in the same institution.

The death is announced of Prof. Luigi Ferri, of the University of Rome, and of Prof. G. Glogau, of the University of Kiel. An Italian committee solicits subscriptions for a monument to Professor Ferri. American subscribers may send names and amounts to this REVIEW, or to the Editor *Rivista Italiana di Filosofia*, via del Govorno Vecchio, 121, Rome, Italy.

Mr. H. M. Stanley has in the press of Swan Sonnenschein & Co., a work titled *Studies in Evolutionary Psychology.*

Dr. Georg von Gizycki, Associate Professor of philosophy in the University of Berlin, died on March 4 at the age of 46.

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